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ORIGINAL ARTICLES.

FALSE DOCTRINE IN THE TREATMENT OF FRACTURES.[1]

BY JOHN B. ROBERTS, M.D.,

PROFESSOR OF ANATOMY AND SURGERY IN THE PHILADELPHIA POLYCLINIC.

It is my desire to call attention to some points in connection with the treatment of fractures which I believe to be errors, but which I think are accepted as axiomatic truths by many members of the profession. That discussion may lead to an interchange of views and perhaps be effective in correcting these erroneous traditions, if they be erroneous, is my object in presenting this paper.

The idea is entertained by many that every fracture of the extremities should be treated by a special splint or apparatus. The simplicity with which fractures are treated by us in the Philadelphia hospitals has often caused surprise to those practitioners who come to us for post-graduate instruction. Their previous teaching or reading has evidently created the mistaken impression that complicated special devices are essential for each locality of broken bone. The fact that treating a fracture is a simple mechanical problem capable of solution by any device that will secure correct apposition and immobilization, while at the same time inflammatory conditions are presented, is not recognized.

The quite frequent use of a bandage, next to the skin, before the splint is applied to the extremity is due to false teaching, and is fraught with danger because of the possibility of its causing unexpected constriction in the event of rapid inflammatory swelling. This primary bandaging has been advocated to prevent swelling and muscular spasm. That it does either to any beneficial extent is doubtful. We possess other and less dangerous methods that are more effectual for such purposes.

It is quite commonly believed that ensheathing callus is one of the essentials of proper union after fracture, while the truth seems to be that ensheathing callus is seldom found except in fractures of the ribs and other fractures where immobilization of the fragments is imperfectly accomplished. A fracture so held in proper coaptation that motion cannot occur heals without ensheathing callus in nearly all instances. Cicatrization goes on in bone wounds essentially as it does in wounds of soft parts.

Early institution of passive motion during the treatment of fractures near joints or involving joints is still insisted upon by many practitioners. One of the greatest sources of anxiety to the young or inexperienced doctor is to know when to begin passive motion. He fears to begin too early lest he disturb the process of union; he dreads to leave it too late lest he have an anchylosed limb as the result of his tardiness. The proper course, it seems to me, is something like this: If the joint is involved in the line of fracture passive motion at an early stage will not prevent anchylosis, but may increase it by causing a greater degree of arthritis; if the joint is not invaded by the fracture line, early passive motion is not needed, because anchylosis will not occur unless violent inflammation of the soft parts arises, which inflammation passive motion is more likely to increase than to decrease. In accordance with this view I advise that no passive motion be made earlier than three or four weeks in any case. The adoption of passive motion earlier than this has often in energetic but injudicious hands given much unnecessary pain, and perhaps in many cases increased the arthritis and subsequent stiffness. The degree of restoration of function possible after articular fractures is only determinable after many weeks. Passive motion should certainly not be commenced while arthritis is acute, and not as a rule until union of the fracture is pretty well accomplished. When it is attempted the occurrence of arthritic reaction is an indication that it must be still longer postponed.

Splints and dressings are often continued too long, and thereby the disability of the patient for attending to his business prolonged. In uncomplicated fracture of the tibia and fibula the patient should be able to go on crutches to his store or office in two or three weeks; provided that a silicate of sodium or a gypsum dressing has been applied. After fracture of the fibula of ordinary severity one week's confinement to the house is sufficient, provided that some supportive dressing be thereafter worn, and crutches used. The usual uncomminuted fracture of the lower end of the radius needs no splint after ten days or two weeks. Although, of course, function is

[1] Read in the Section on Anatomy and Surgery, in New Orleans, April 28, 1885.

not perfectly restored, the hand and fingers can be used for many purposes involving little muscular effort. While not wishing to advocate rapid convalescence when caution requires a few days' additional confinement, I hold that it is improper to keep a patient from pursuits that need his attention, merely because of the traditional idea that a fracture means six weeks' enforced idleness. Loss of money, mental anxiety, and continued disappointment of business connections are penalties too great to endure because of a tardy convalescence insisted upon by routine practice.

It is false doctrine that still insists upon the great risk incurred when a closed fracture of the cranium is converted by the surgeon into an open one, in order to explore supposed dangerous characteristics which, if present, threaten life from probable secondary encephalic inflammation. The possibility of septic infection is increased, I admit, but so little that the danger of obscurity in diagnosis and consequent erroneous treatment is often much greater.

Fractures of the nose have long been, and still are, often treated by useless dressings. The conventional application to broken nasal bones is a strip of adhesive plaster placed across the bridge of the nose with the idea that it will by its adhesion to the skin hold the broken fragment upward, and prevent depression of the nasal arch. That it is quite impossible for a flexible tissue like adhesive plaster to act in this manner will be recognized with the mere statement. If comminution tends to allow displacement, the plaster will not give sufficient rigidity to obviate the tendency. If it does no good, why disfigure the patient by making him wear it? The proper method of retaining fragments in position when great tendency to displacement exists, is by transfixing pins; but as the object of this paper is not to deal with plans of treatment, I shall not discuss the procedure at this time. Another custom quite prevalent is to put tubes in the nostrils after nasal fractures or operations, when we all would prefer mouth breathing to wearing nasal canulas, which are unsightly, uncomfortable, dirty, and which as a rule soon become clogged. Breathing through the mouth for a few days is easily borne when an acute nasal catarrh is contracted; therefore its performance after nasal injuries is not intolerable. If a plug is required in the nostril to maintain position of the fragments let it be introduced, and let it be a tube if you choose; but it will usually become clogged and offensive. A solid plug will in most instances be more cleanly.

Deformities of the nasal bones and cartilages often become permanent after fracturing injuries because it is believed that there is little relief for the displacement. Properly conducted surgical treatment at the beginning or operative measures afterward will relieve much of the unhappiness resulting from unseemly lateral deviations and irregular distortions of the nose. The importance of this feature in the facial lines renders defects in conformation so noticeable that in hypersensitive persons mental characteristics are often due to nasal deformity in childhood. It may be remembered that a commander of ancient times gave the order "aim at their noses" knowing that the enemy feared facil disfigurement more than actual death. The false doctrines prevalent concerning nasal fractures should therefore meet an early overthrow. It is more important to treat a broken nose well than a broken leg.

The use of the axillary pad in treating fractured clavicle is of little or no value. The important factor in the treatment is to so fix the inferior angle of the scapula, that the scapula cannot slide forward upon the lateral wall of the thorax, as it tends to do, because the clavicle, which is its only bony attachment to the trunk, is broken. Displacement of the fragments in broken clavicle is to be prevented by steadying the lower end of the scapula and not by an axillary pad which is ineffectual as a fulcrum against which to use the humerus as a lever to throw the acromial end of the clavicle outwards and backwards. The axillary pad is useless unless large and hard; if large and hard it cannot be worn without discomfort, that would usually be accompanied by danger of injurious pressure to soft parts.

The employment of an internal angular splint for fractures in the vicinity of the surgical neck of the humerus is founded on false premises. The axillary muscles prevent the upper end of the splint extending high enough into the axilla to control the upper fragment. Hence the splint does not keep the upper fragment at rest, and, by its projection beyond the elbow or hand, gives more leverage by which unexpected blows may cause motion of the lower fragment. It is better to use the thorax as a splint, and bandage the arm to the chest with perhaps a small amount of packing, such as absorbent cotton or lint, in the axilla to steady the upper fragment.

The fallacy of treating fractures of the condyles of the humerus by anterior or posterior rigid angular splints, and thereby causing deformity and disability by impairing the external angular deviation of the axis of the upper extremity, was shown by Allis four years ago (Transactions of Medical Society, State of Pennsylvania, 1881). Yet this is probably the method by which such fractures are treated by most of the members present to-day. The loss of the carrying power of the arm after treatment of condyloid fracture by such splints, is, I have no doubt, a common experience, though many may not have recognized the cause.

In fractures at the middle of the forearm, interosseous pads are seldom, if ever, required if the fragments are moulded into proper position and the forearm is put in a position midway between pronation and supination. The interosseous space cannot easily be preserved by the

use of an interosseous compress, if moulding and the position mentioned will not do it. The bones are too much enveloped in muscles to be controlled by a superficial pad, even if it is long and narrow and hard. At least such will be found the case in most instances. A lamentable practice, founded on false doctrine, is the use of a straight, that is flat, splint for the ordinary fracture of the lower end of the radius. The palmar surface of the lower third of the radius is concave, therefore the splint must be curved. Yet the practice of employing a Bond's splint or some other form of flat splint is common. A convex splint or a splint with a hard pad, with a convex upper surface, is the only form of a splint proper to use on the palmar aspect of the fracture. A straight splint will do well on the dorsal, but not on the palmar surface. Use, therefore, either a curved palmar or a straight dorsal splint if you desire cure with the least possible deformity. The stiffness of fingers and deformity, so frequently seen after these fractures, are due to imperfect reduction of the fragments and improper splints. In many cases reduction without the application of any splint will give better results than reduction with the use of a flat splint.

The teaching that fractures of the shafts of metacarpal bones should be treated by palmar splints, may not be universal, but it is, I think, very common. In oblique fractures the deformity can often be overcome best by continuous extension adjusted to the finger by means of adhesive plaster, as is done in fractures of the femur. Strips of adhesive plaster attached to the finger and an extending cord, preferably of rubber, fastened to a splint placed under the wrist and palm and extending beyond the finger tips is a serviceable dressing for correcting overriding in metacarpal fractures.

The habit of measuring the length of the lower extremities in suspected fracture of the femur is founded on a mistaken impression that the legs are of the same length. The frequent asymmetry in length of normal limbs has been so often demonstrated that it is surprising to see surgeons constantly employ this as a method of diagnosis. Even if the legs were known to be of equal length the measurement would probably be inaccurate, because of the difficulty of avoiding tilting of the pelvis and of applying the tape to exactly similar points on each side. When it is known that normal legs differ in length, the folly of placing any diagnostic dependence on the figures obtained is apparent.

The disability liable to follow fractures of the femoral neck in patients beyond middle life, is not as great as it is often stated to be. Whether this is due to a mistaken diagnosis between intracapsular and extracapsular fracture, I know not; but I am convinced that the impression prevails to a great extent among the profession, that fracture of the neck of the femur in an old person means almost helpless lameness. Such is not the case. Very good use of the limb quite frequently happens.

In oblique fractures of the legs with overlapping reduction can at times be facilitated by tenotomy of the tendo Achillis. This means of overcoming displacing muscular action is perhaps not as often resorted to as it should be.

Extension by traction applied to the head and legs should be better known, I think, as a possible method of reducing fractures of the vertebræ. In many cases it will do no good, but in others it may.

The aversion to applying coapting hooks to the patella and olecranon, when apposition is otherwise impossible, is, in my opinion, the result of false teaching and observation.

There are many points of this character upon which I might dwell, but I have said enough to indicate my disbelief in many of the popular traditions of surgical practice. I shall now wait to hear in the discussion that will follow, what justification for my beliefs or disbeliefs I can get from the practical men here present.

HÆMORRHAGIC MALARIAL FEVER AS IT OCCURS IN ALABAMA.[1]

By JEROME COCHRANE, M.D.,

STATE HEALTH OFFICER OF ALABAMA.

Ordinarily it is very unwise for anyone to attempt to give the description and treatment of any disease which has not been made familiar to him by long practice at the bedside, and in the beginning of my paper I desire to state frankly that I have no knowledge at all of hæmorrhagic malarial fever based on personal observation. Circumstances, however, have led me to make a tolerably thorough study of this disease, and my present contribution to its natural history seems to me to be warranted by these two considerations: 1. That I have accumulated a large amount of material that has not been heretofore in any proper sense of the word published; and 2. That I shall confine myself strictly to the analysis and arrangement of these new sources of information to the entire neglect of the literature of the subject. A very brief statement will show how I came in possession of the information here alluded to.

In my account of the epidemic fever which prevailed in 1883 in Brewton, a small town in Escambia county, Alabama, I have stated in some detail the problem of differential diagnosis which had to be solved there. I had for many years been familiar with yellow fever, and to me it seemed perfectly clear that the fever at Brewton was yellow fever. The physicians of Brewton had been for many years familiar with hæmorrhagic malarial fever, and they asserted with persistent emphasis that the Brewton fever was hæmorrhagic malarial fever. At that time I had no circumstantial and critical knowledge

[1] Read in the Medical Section at the Thirty-Sixth Annual Meeting of the American Medical Association.

of hæmorrhagic malarial fever, having in all my professional life seen only one case of it. I had read in a casual way the papers which had been published with regard to it in the Transactions of the Medical Association of Alabama, and had on several occasions heard it discussed during the annual sessions of said Association. But all this had given me only a vague general knowledge of the malady, and not that minute, definite, and circumstantial information of the concourse and succession of symptoms which is needed for purposes of diagnosis in the presence of an emergency such as that which confronted me at Brewton.

As a consequence of this embarrassing experience I made up my mind to make as careful a study of hæmorrhagic fever as I could, with the special object in view of establishing the data upon which any medical man of ordinary skill might be able with confidence and certainty to distinguish this said hæmorrhagic malarial fever from specific yellow fever. To this end, accordingly, in the winter of 1883–4 I distributed among the physicians of Alabama one thousand copies of a circular letter requesting them to furnish me all the facts in regard to this fever they had been able to glean from their personal observation of it. To this circular letter I received forty-four responses, some of them very brief and some of them quite elaborate. My own lack of clinical acquaintance with the disease makes it impossible for me to undertake to estimate the comparative values of the statements and opinions of all these witnesses, and as the result of my analysis of their testimony I can not venture to attempt to draw such a picture of the malady as will show all the minute details which characterize it under its several forms and in the several stages of its evolution. The most that I can do is to present such a general outline of it as will exhibit its larger features, and to call attention to such of its characteristics as seem to be established by the substantial agreement of many witnesses.

The circular letters here referred to, together with a discussion of them, of which this analysis is largely a rehash, were printed in the Transactions of the Medical Association of Alabama for 1884, pages 495–619.

In the fall of 1884 it came to my knowledge that cases of hæmorrhagic malarial fever were occurring in several counties of Alabama, and with the view of clearing up some of the disputed questions in regard to it I requested my medical friends to keep careful clinical records of their cases, and to furnish me with specimens of the urine and the blood for chemical and microscopic study. At the same time I insisted very urgently that no opportunity for post-mortem examinations should be allowed to pass without utilizing it to the utmost advantage.

As the result of this effort I obtained: 1. clinical reports of three cases, two from Dr. D. S. Hopping, of Letohatchie, in Lowndes county, Alabama, and one from Dr. R. D. Webb, of Livingston, in Sumter county, Alabama, which I gladly include in my paper.

2. Several specimens of the characteristic red urine, together with other specimens passed a little before the appearance of the red urine and a little after its subsidence. It was my good fortune to be able to obtain the aid of Dr. G. M. Sternberg, of the United States army, in the study of this urine, and his valuable report of the results of his study is also made a part of this paper.

I failed to get any specimens of blood. I also failed to get any opportunity to make a post-mortem examination.

Dr. Webb's Case.—Dr. D. S. B., aged 30. Practises in a swampy, alluvial district, in which in the fall of the year there occur the several forms of malarial fever. In the fall of 1883 he had repeated attacks of malarial fever, one of which approximated the hæmorrhagic variety, the surface of the body being considerably tinged with yellow color, and the urine highly colored, probably by hæmatine, but at this time it was not examined.

On November 6th, 1884, he had a slight chill followed by fever. On the 7th the chill and fever were repeated, but still without alarming symptoms, and he continued to attend his patients, neglecting to take anything to arrest the chills.

On the morning of the 8th (9 A.M.) he had a third chill, which was more severe, and which lasted for three hours. As the chill began to disappear and febrile reaction set in, he had a large discharge of very dark-colored urine, supposed by him to be hæmaturic. By 4 P.M. his fever had abated and he rode in his buggy to Livingston, a distance of ten miles. He consulted me at my office immediately on his arrival. His appearance was sallow, and he appeared somewhat debilitated. His fever was now nearly or quite off, and his urine clear. I advised him to at once take his bed and place himself well under the influence of quinine by 8 A.M. the next morning.

November 9th I was absent from town and did not see him until the morning of the 10th. I learned on my return that at 5 A.M. (on the 9th), before he had taken quinine enough to be under its influence, he had another chill, followed by a copious discharge of very dark-red urine. His fever continued during the day and until the middle of the night. The early part of the day his urine whenever voided was deep-red or dark. During the evening it gradually lost its dark color, and by the middle of the night was of natural color. He was seen by Dr. Arrington on the 9th, and it was agreed that he should take thirty grains of quinine by 4 o'clock A.M., commencing at 10 o'clock at night.

November 10th, A.M., I found him clear of fever, urine of natural color, and skin slightly tinged yellow. I should mention that his bowels

had been acted upon by calomel and podophyllum. He had been very much nauseated during the past night, and feared he had not retained his quinine. This morning (10th) 8 A.M., he was still very much nauseated and occasionally ejected from the stomach a viscid, watery fluid of a light-green or bluish color. His stomach revolted at the idea of any further medication, and it was thought best to trust him during the day without anything else than such remedies as might allay the nausea. For this purpose small doses of carbolic acid and morphine with drafts of hot water, which was grateful to the patient, were tried, with, however, but little good results. The nausea and depression continued until 2:30 P.M., when he had a return of fever, with very slight indications of a chill. The urine, which was very copious, became again very dark-red. The nausea was distressing, the skin became distinctly yellow, and the patient ejected considerable quantities of viscid, bluish fluid from the stomach. The patient had a distressed appearance, and was growing quite feeble. These distressing symptoms continued until midnight, when there was an abatement in the fever, and the urine became again nearly of natural color.

November 11th. Another attempt was made to place the patient under the influence of quinine during the night. The quinine up to this time had been given in capsules. Fearing it was not taken into the system, I had pills prepared with dilute sulphuric acid and glycerine to render them more soluble.

During the night he got twenty-five grains of quinine prepared in this way. Slept but little and was distressed throughout the night by nausea. This morning (11th) he got hypodermatically one-third of a grain of morphine to control the nausea. His fever returned at 2 P.M. (temperature 102), free discharge of hæmaturic urine, nausea and vomiting severe. His fever did not abate as usual by midnight, and the temperature remained at 102 F. The urine also continued dark.

November 12th. Fever not abated, severe nausea, and urine still dark. Fearing that the quinine was still not retained or not absorbed, I determined to use it hypodermatically, with atropia and morphine. At 6 A.M. he got eight grains of quinine in pills, with morphine, grains one-quarter, and sulph. atropia, grains one-fiftieth, hypodermatically. At 10 A.M. he got hypodermatically quinine, grains eight, and at 12 o'clock quinine, ten grains, in the same way; at 8 P.M. he got one-fiftieth of a grain of sulph. atropia. Has not voided his urine since last evening. The patient was now drowsy and somewhat comatose, and his condition seemed almost hopeless. Eight ounces of urine were drawn by catheter. The color was rather darker than natural, but clear, and temperature 100. No exacerbation of fever to-day.

November 13th. Got bitart. potassa and acetate potassa during the night. This morning the patient is weak, but mind is clear, and he is more cheerful. Drew off by catheter ten ounces of clear urine, of natural color; got one-fiftieth of a grain of sulph. of atropia. Temperature 99. Ten ounces more of natural-looking urine drawn off, P.M.

November 14th. Patient clear of nausea; slept well; twelve ounces of urine drawn off this morning; bowels acted upon by sulph. magnesia.

November 15th. Patient convalescent, but very feeble, with a liberal diet, stimulants and tonics; was up in a week, but it was fully a month before he recovered his strength.

The urine, which was frequently tested during the attack, gave decided indications of blood whenever it was of a dark color; whenever it was natural in color no indications of blood, but albumen in notable quantity was never absent until several days after convalescence. Specimens were furnished Dr. Cochran, and the analysis by Dr. Sternberg will be furnished by him.

Remarks.—This was a case of distinctly intermittent hæmorrhagic malarial fever. I had never before failed to arrest such a case on the second or third day by quinine. The first day after he was seen (9th) he did not get his quinine soon enough. The next day he seems to have been brought partially under its influence and the paroxysm postponed from 5 A.M. to 1:30 P.M.—an effect I have frequently noticed in ordinary intermittent fever when a patient is brought partially under the influence of quinine, but not sufficiently so to arrest the attack.

On the 11th, I am notified that from nausea the quinine was not retained, or if retained, the condition of the stomach was such that it was not absorbed.

On the 12th, he got quinine hypodermatically in conjunction with atropia and morphine, and the fever, which had now become more continued and persistent, was promptly arrested and the patient steadily convalesced.

A single word in regard to the absence of blood-corpuscles in the urine examined by Dr. Sternberg: In your letter of yesterday (March 24, 1885) requesting an account of this case you say: "To the cases I will add Dr. Sternberg's investigation of the urine, which was thorough, chemical, microscopic, spectroscopic. He found no blood-corpuscles in any of the specimens, but found the coloring matter of the blood especially hæmoglobin."

This is exactly as I should expect. I have no doubt blood-corpuscles existed in this urine at the time it was voided. An imperfect examination with a deficient microscope made at the time satisfied me of this fact.

Almost an identical state of facts occurred with a specimen sent by me in November, 1875, to Dr. James Tyson, of Philadelphia. After a careful examination of the specimen, he writes: "The urine contained no blood-corpuscles, but the coloring matter was really hæmatine, as was proved by obtaining the hæmine crystals." This

was on November 9. I at once wrote him that I could but believe the absence of *blood-corpuscles* was due to the fact that they had been changed by the then alkaline character of the urine."

In reply to this, he wrote on November 21, 1875: "I think the suggestion a very reasonable one. It is well known that blood-corpuscles are soluble in alkaline, and disappear very soon in alkaline solutions. In strong alkaline solutions they are immediately dissolved, under the eye, and I have often watched them disappear under the microscope, under these circumstances."

Again on September 21, 1876, he writes in acknowledging receipt of my essay on "Hæmorrhagic Malarial Fever," published in Alabama Transactions, 1876. "I have recently made an observation which confirms our notions as to the cause of the absence of blood-corpuscles in some instances from urine. About three weeks ago a specimen of urine was brought to me, evidently containing blood, in which the microscope proved the presence of thousands of corpuscles on a single slide. I examined it again yesterday and found the corpuscles almost totally gone."

Here, then, I believe, is the solution of the fact that Dr. Sternberg found no blood-corpuscles. The urine when examined by him had become *alkaline* and had dissolved the corpuscles.

Dr. Hopping's Cases.—Case of Hampton H. Called November 20, 1884, to see Hampton H., white, aged 17 years. A stout, tough boy, but dwarfed by hard work and malarial saturation of long standing. Has had many attacks of chill and fever during the past three years, escaping not always during the winter months. His present attack is of the double tertian type. The surface soil surrounding his residence is of a sandy loam with a tenacious red sub-soil very retentive of moisture—conditions most favorable for the generation of malaria. Of the forty-four cases that I have treated, forty have occurred where such soils abound.

His chill of yesterday evening attracted no particular attention, but that of this morning, at 10 A.M., caused unusual restlessness attended with rigors.

I reached his bedside at 2 o'clock P.M. with the above history. Pulse 108, full and strong, temperature 104 2-10, skin perspiring freely; urine of the port-wine color, is freely and frequently discharged, not, however, so blood-like as in majority of such cases; the icteroid hue very perceptible, but not deep. Tongue furred, suffers very little with nausea, spleen enlarged and tender on pressure, no enlargement or tenderness of liver. Mind clear, but watchful and apprehensive. Ordered calomel, 12 grains, and Dover's powders, 6 grains, made into three portions and to be taken at once—one every three hours—and ordered 18 grains of quinine in six portions, beginning at 10:30 P.M., and repeat every two hours. Bottles filled with hot water were unremittingly applied next to his person to encourage the diaphoresis.

November 3rd, 11 o'clock A.M. At 6 o'clock this morning temperature was 98; at this hour 101, pulse 115, full and rather gaseous, skin perspiring freely. The bowels have been moved twice by the medicine, the last very dark and consistent; urine is still freely discharged, less, though, than yesterday, and not so highly colored; discoloration of skin something greater. He is now fully under the influence of the quinine. Ordered tincture of iron 20 drops, with 2-grain solution of chlorate-potash every three hours and repeat; 3-grain doses of quinine at 12, 3, and 6 o'clock in the morning. Milk punch, eggnogg and chicken water *ad libitum*, and castor oil, ½ ounce, if bowels are not moved by 12 o'clock to-night.

November 4th, 1 o'clock A.M. His father writes, Hampton has had a chill and that he is totally unmanageable, fever very high. I replied by note ordering a hot foot bath and to sponge his face, neck, and chest with the same and apply cloths rung out in hot water to chest, stomach, and bowels. This was promptly done, and I reached his bedside at 7:35 A.M., his father informing me that my prescription gave instant relief. Has taken the 9 grains of quinine; pulse now 116, greatly moderated in force and volume; temperature 99 2-10. Bowels moved by the oil. Discharges black and tarry, skin and urine again more deeply colored. To facilitate the diaphoresis and to prevent chill, I prescribed camphor gum, grains 2, sugar, 2 ounces, and chloroform ℥j. rubbed well together, and water sufficient added to fill an ounce vial. Dose, a teaspoonful every hour for four hours. Tongue is, as it has all the while been, heavily furred. Ordered calomel, 3 grains, at 11 A.M., again at 3 P.M. Quinine again to-night at 12 o'clock and at 3 and 6 o'clock. Six o'clock P.M., temperature 99 4-10, pulse 98.

November 5th, A.M. Had a very restless spell last night, which was again promptly relieved by the hot foot bath and hot sponging. Pulse 126. Quinine, grains 2, every two hours, until three doses are taken, to be taken during forenoon of to-morrow. Tincture of iron and potash as before.

November 6th. Found Hampton still improving, color both of urine and skin improved. Temperature 98, pulse 100. Took iron and potash as before and quinine, 6 grains, in three portions, during the forenoon every day.

Temperature and pulse range much the same as on yesterday, manifesting a gradual improvement. Under the quinine, iron, and supporting diet, improvement was evident until the 12th. On this day during the afternoon, his temperature ran up to 104, cheeks red, urine again red. Ordered calomel, 8 grains, in two portions, with Dover's powders and quinine, 6 grains, to be taken from 12 to 8 A.M. in two portions.

November 13th. Had a rigor at 10 P.M. last night and a decided chill at 6 A.M. this morning. Urine highly colored. Tongue rather dry and thirsty. Iron and quinine, the latter 6 grains

daily. He is quite nervous and restless. The hot sponge bath of face and chest gives relief and quietude. A full dose of oil and fifteen drops of turpentine is given. Diet, milk punch and raw eggs and sugar.

November 14th. 11 o'clock A.M., temperature 100 8-10, pulse 100. Bowels acted well three times since yesterday. Had a better night than anticipated. Urine much paler, yellowness of skin fading. Has some appetite, no nausea. Afternoon, temperature 100, pulse 108. Treatment continued.

November 15th. Pulse 88, temperature 100 6-10. Had a chill yesterday evening, but was followed with light fever; urine and skin much the same.

November 16th. Had a good night. Temperature 99 8-10, pulse 95. Takes tinct. eucalyptus and tonic of iron, quinine, and strychnia. The condition of the children seemed, under the watchful care of the parents with tonic medication and supporting diet, not to require my daily visits. Hampton did well until the twenty-first day of his illness, when the chill again returned. This, like the former, was attended with increased discoloration of urine and skin, and anæmia more evident, and again broken up by calomel and quinine, the latter increased to twelve grains daily. Improvement again took place. After arresting the chill the doses of quinine were again reduced. And on this, his twenty-eighth day, a chill was clearly manifested. His bowels had been neglected. Oil and turpentine given, and repeated when twenty-four hours without an action and the chill day anticipated by increased doses of quinine, enabled him to miss chill on the thirty-fifth day and convalescence assured.

The only comment I would make on this case is to note the persistent malarial taxæmia, as shown by the periodical return of paroxysms on the 14th, 21st and 28th days of his attack, and the effect of quinine in preventing its return when given in anti-periodic doses, and the certainty of paroxysmal returns when given in small doses, viz.: Three to six grains during twenty-four hours. In respect to the opinion of many learned in the profession, I was induced, after breaking up a paroxysm, to fall back on small doses. I did so with fear and trembling, for my experience had too often made it evident that a chill would come on hebdomadal periods. And as in others, so in this case, I was constrained to return to my first love (quinine in large doses), and to her my patient is indebted for his being to-day.

Such diversity of opinion as to the value of the great anti-periodic may possibly be reconciled on the hypothesis that the cause is more potent in some localities than in others. With my views as to the certainty of the returning paroxysm and the virtues of the anti-periodic, it would be criminal to withhold it.

Case 2.—November 4th, 1884. G. H., age 14, sister of Hampton, has had many attacks of malarial fever during summer and fall. The present attack is of the double tertian type. Had a chill at 2 o'clock this P.M., and her urine, which was passed frequently and copiously, was observed to be dark-red, skin soft, and perspiring freely, and jaundiced, and tongue deeply furred. At this hour (11 o'clock P.M.) her temperature is 102 2-10, and pulse 126. Gave chloroform and camphor mixture to stimulate and encourage the diaphoresis, and tinct. iron, twenty drops, and chlorate pot. solution every two hours (two grains at each dose).

November 5th. Soon after I left my patient last night she had a hard rigor at 2 o'clock, A.M., fever following of about one hour's duration. Urine dark-red, skin very yellow, respiration sighing; nauseated, and occasionally vomits the iron.

Continued chloroform mixture, and the iron and potash as before, and quinine at 12, 3, 6, and 9 o'clock this P.M. in three-grain doses, and a half ounce oil, at 10 o'clock this A.M. She can not retain anything on her stomach, P.M. At 12 o'clock to-day her temperature was 101 2-10, and at 5 o'clock P.M. 103 2-10. Skin very yellow, urine very dark-red, freely and frequently discharged. Every expected paroxysm is anticipated by drachm doses of the chloroform mixture, flying mustard plasters to spine, stomach, and extremities. Bowels have been moved at 11 o'clock to-night after repeating oil at 6 P.M. Then took ten grains quinine in four portions, one every two hours.

November 6th, 9:40 A.M. She had a bad, restless night, retained the quinine with difficulty, and when vomited was immediately repeated. She is extremely weak, anæmic, and jaundiced, not so much urine discharged, and it is perceptibly paler, pulse varying from 130 to 140, and very weak. Temperature 96 6-10, respiration labored and sighing, the breath is cold, restlessness very great, and nausea too great to take nutriment. Whiskey toddies with lemon juice, and flying sinapisms pressed as before. Three o'clock P.M., pulse thready and hard to count, about 140. Temperature 96 4-10, restlessness extreme, will not permit her hands and arms to be covered, breath very cold to my hand. Four o'clock P.M., pulse 150. Slightly more febrile, delirious, right hand and arm cold, skin deep-yellow, urine passed in bed, does not discolor the sheets greatly, tongue colored black by the vomiting of bile and iron, and very dry. She took toddies and milk punches through the night very reluctantly, and when vomited were repeated. External stimulants also continued. Pulse through the night varied from 140 to 150, extremely weak and hard to count. She is delirious, talkative, and hard to control.

November 7th, A.M. At 5 o'clock A.M. the stimulants were more energetically pressed, and she slept a little, the first in two nights. Pulse at that hour 140, temperature 96 6-10.

Prostration very great. Tongue dry and black. No move from the bowels; still delirious and talks incessantly, restlessness if possible still greater, hands and arms cannot be made warm by hot flannels.

Stimulants continued. Just here I felt deeply the responsibility of determining the propriety of administering a mercurial to a delicate patient so near unto death. I feared that I had already waited too long, and saw plainly that something had to be done or dissolution would result. I gave the mercury (calomel 11½ grains with Dover's powders every three hours until four portions were taken—beginning at 8 A.M., At 10 A.M. this morning her temperature is 98, and pulse 140, and attempted to give quinine in 1½-grain doses during the day, but stomach would not tolerate it. Six o'clock P.M., has retained the calomel. Temperature 97 1-10, pulse as near as can be counted, 140. Still delirious, talkative and will not permit her hands and arms, which are still cold, to be covered. Urine in color and quantity apparently normal. Skin still deeply discolored.

Eleven o'clock, P.M., condition much the same; if any change, it is for the better. Stimulants both internally and externally continued.

November 8th. Two o'clock A.M., had a small action from the calomel; 10 o'clock A.M., temperature 98 6-10, pulse 132. Had many good naps of five or ten minutes' duration. Tongue a little moist on sides, and dry and black on top.

Twelve M. Pulse 120, temperature 99 1-10.

Three o'clock, P.M., gave oil, a tablespoonful.

November 9th. Had an extremely restless night, extremely impatient. Bowels acted many times, all of them as black as tar. She is given toddy during every effort to go to stool. Temperature this A.M. is 98, pulse 110, febrile, tongue dry on top, thin and pale, countenance anæmic and denotes great weakness. Her short naps disturbed by frightful dreams, and while asleep her countenance denotes pain, and on waking complains that she is so hot; and will talk, despite all of our efforts to restrain her; and, though all of her symptoms have ameliorated, she is at times delirious. Skin and urine apparently natural. The latter when first discharged is clear, but soon becomes turbid. Treatment, stimulants continued, tincture of iron in solution of chlorate of potash, chicken soup and bottled soda water *ad libitum*. Since medicine acted well nausea has subsided. A turpentine wash for the mouth relieves the dryness.

Six o'clock P.M., is doing well.

November 10th, 3 o'clock. Has rested well since last evening, temperature at 7 o'clock this morning was 99, pulse 116. At 11 P.M. pulse 126. At this hour it is 106, hard and quick, respiration less noisy and talks less, no delirium. Bowels have continued to act six or eight times during past twenty-four hours—discharges still dark as gunpowder, and tarry; urine clear and copious, passed three times in last twenty-four hours. Tender over region of liver and bowels. Supporting treatment continued.

November 11th, P.M. Had a comfortable night, pulse ranged from 108 to 112 during last night, temperature not taken, instrument broken. Bowels continue to act freely, black and tarry. This P.M., pulse 102, temperature 100 8-10. Gave turpentine, four drops, and twenty drops tincture of iron every six hours. Appetite improved, takes chicken soup and milk punch in small quantities with relish, also raw eggs and sugar; continues the bottled soda. Tenderness over liver has disappeared.

November 12. She still holds her own, temperature 101 4-10, pulse 106, eats and sleeps well. Has from four to six moves from the bowels during twenty-four hours, thick and black as before. Treatment continued.

November 13th. Temperature 102, pulse 104. Bowels acted six times in past twenty-four hours, the same black, thick, and tarry discharges, and in large quantities. Takes three grains of quinine during the morning every three hours until nine grains are taken. Iron and chlorate of potash every six hours. Temperature at 7 P.M., 100, pulse 90.

November 14th, A.M. Rested well last night and feels well, temperature 99, pulse 90. Treatment continued. Temp. 100, pulse 88 this P.M.

November 15th. Pulse 86, temperature 100. Tonic doses of iron, ten drops, quinine, one grain, and strychnine, gr. $\frac{1}{60}$, three times a day.

November 16th. She is apparently doing well, though her temperature is 102, pulse 98, cheeks red, less thirst, and tongue more moist. Still takes nourishment with relish, complains of no pain or tenderness. Convalescence from this date though slow, under the iron, quinine and strychnia. Tincture of eucalyptus was sure.

DR. STERNBERG'S REPORT.

It is hardly worth while to give you a detailed account of the examinations which have been made of the various samples of urine sent to me by yourself and your correspondents, but I will state briefly the results of these examinations.

Samples were received:

November 10th. One sample of port-wine-colored urine, acid reaction, highly albuminous, contains epithelium from bladder, granular tube casts, and micrococci in chains.

November 12th. One sample, port-wine color, acid, albuminous, contains epithelium from bladder, and tube casts (marked Gussie 2).

November 18th. A box containing several samples was received. One of them was of the characteristic port-wine color and received special attention. Like the other samples it was acid and highly albuminous.

Other samples sent with this had been kept for a considerable time, and were specimens obtained from the patients Gussie and Hampton, which had been passed first before a hæmaturic paroxysm. One of these samples was of a pale straw color, acid reaction, and non-albuminous. It

contained an abundant deposit of uric-acid crystals. Another sample, marked Gussie No. 1, had a pale-yellow color, was albuminous, and contained epithelium from the bladder, and granular tube casts. Another specimen had a light-pink color, and contained an abundant amorphous sediment. It was non-albuminous.

November 21st. Two samples received and two broken bottles. The bottles not broken contained urine passed in advance of a paroxysm. One sample, marked "Gussie No. 3," was neutral, non-albuminous, and contained uric-acid crystals. One sample marked "Hampton No. 2" was acid and slightly albuminous.

December 29th. Two bottles of bloody-looking urine were received. No. 1 was neutral, highly albuminous, and contained an abundant amorphous sediment, also an abundance of micrococci and bacilli. No. 2 was alkaline, highly albuminous, and also contained an abundant amorphous sediment, and numerous bacilli and micrococci. These samples had evidently undergone putrefaction changes, and were of no value.

I have examined every specimen sent, with reference to the presence of blood-corpuscles, and have not in a single instance found any. This absence of blood-corpuscles is, in my opinion, satisfactory evidence that the color of the urine is not due to hæmorrhage from the kidneys, for blood-corpuscles placed in normal urine may be recognized at the end of ten days or more, and after the urine has undergone alkaline fermentation. That the color is due to the presence of hæmoglobin is proved by spectrum analysis, and by the formation of hæmin crystals when the port-wine urine is treated with glacial acetic acid and chloride of sodium, and heat is applied.

Dr. H. Newell Martin, Professor of Biology in Johns Hopkins University, has kindly applied these tests to several of the samples sent, and has demonstrated the abundant presence of hæmoglobin. It seems to me, therefore, to be proved that the abnormal appearance of the urine is due to an excretion of hæmoglobin which occurs during the paroxysm, and that we have to deal with an intermittent hæmoglobinuria. This being the case, the disease in which this is the most prominent symptom is incorrectly designated when spoken of as "malarial hæmaturia," or as "hæmorrhagic malarial fever."

Various questions suggest themselves with reference to the immediate and remote causes of this intermittent excretion of blood-pigment, but these can only be settled by extended researches, and, owing to other engagements, I have not been able to give the time to this investigation which the importance of the subject demands. Physiologists have shown by experiment that the injection of bile salts into the circulation of a dog causes the red corpuscles to lose their hæmoglobin, and that hæmoglobinuria occurs as a result of such injections. This suggests the possibility that the retention of bile salts in the blood may be the immediate cause of hæmoglobinuria in cases of so-called "hæmorrhagic malarial fever."

NATURAL HISTORY.

The natural history of hæmorrhagic malarial fever, so far as it can be drawn from the witnesses I have indicated, may be summed up in the following brief propositions:

1. That hæmorrhagic malarial fever originates only in malarial regions, although the morbific paroxysm sometimes may occur after the removal of the subject of it to a healthy locality.

2. That the poison which causes hæmorrhagic malarial fever is the same as that which causes ordinary intermittents or remittents. But a few of my correspondents are inclined to doubt this, and to believe that the specific etiological factor in this disease is not identical with that which produces intermittent and remittent fever of the usual malarial types.

3. That hæmorrhagic malarial fever may manifest itself in three different forms, namely, (*a*) the intermittent, (*b*) the remittent, (*c*) the congestive; to which may probably be added a fourth form, (*d*) the quasi-continued. In this respect this fever follows the analogy of the ordinary malarial types.

4. That the congestive form of the malady is almost necessarily fatal under all plans of treatment; the remittent form extremely dangerous, with the deaths in excess of the recoveries; the intermittent form less dangerous than the two others just mentioned, with the recoveries in excess of the deaths; while the quasi-continued form is comparatively rare, and probably the result of inflammatory visceral complications.

5. That negroes and mulattoes are comparatively exempt from the disease in any of its forms; being, indeed, less susceptible to this fever than to yellow fever; and the rate of mortality of the cases that occur among them is very much less than that among the white cases.

6. That in the immense majority of cases it attacks persons who suffer from malarial cachexia, and who have a history of chronic chills. But in a small minority of cases it attacks persons who, while they have been much exposed to malarial influences, have still remained apparently well up to the moment of the malignant outbreak.

7. That one attack affords no guarantee against another, the same person often suffering successive attacks in successive seasons, or even in the same season.

8. That the paroxysms usually begin with a chill, which is often of protracted duration, which occurs frequently during the afternoon or in the night, and which is accompanied or speedily followed by troublesome bilious vomiting, by the characteristic red urine, and by icteroid discoloration of the eyes and skin.

9. That the febrile reaction, as marked by the temperature and the pulse rate, is not usually of very high grade. But there seem to be some

exceptions to this rule in the intermittent and remittent, and especially in the quasi-continued forms, in which both temperature and pulse are dominated by intercurrent complications.

10. That while the fever continues the skin is apt to be harsh and dry, although this rule is not universal, and that the occurrence of free, and even profuse, diaphoresis is a symptom of favorable omen.

11. That the bowels are usually constipated, the portal circulation obstructed, the liver congested and torpid, and the whole system deluged with bile, and that free bilious purging seems always to afford some measure of relief.

12. That the bilious vomiting makes its appearance early, is attended by distressing nausea, and is usually copious and persistent, the vomited matters ranging through many shades of color—yellow, green, black, and even blue.

13. That the black vomit of this fever is usually altered bile, not altered blood, and is free from the coffee-grounds appearance so characteristic of the vomit of yellow fever; but a small number of cases seem to be marked by black vomit not distinguishable by mere inspection from that of yellow fever.

14. That when this black vomit occurs at all it almost always occurs at the beginning of the attack, and is always preceded by the appearance of red urine.

15. That the icteroid discoloration of the skin and eyes, which extends also to the cellular tissues, usually comes on with the initial chill; deepens very rapidly in intensity; may clear up more or less during the remissions and intermissions of the fever, to increase again with the successive exacerbations; may grow less toward the end of protracted fatal cases; and sometimes disappears completely after death.

16. That the characteristic red urine usually shows itself with the initial chill, and always in an early stage of the paroxysm; is at first very profuse; may diminish or disappear in the remissions and intermissions of the fever, to reappear again in the successive exacerbations; may clear up and grow less abundant toward the end of protracted fatal cases; and there is often toward the end of fatal cases complete suppression of all urinary secretion.

17. That the red urine, which is the special characteristic symptom of this fever, is highly albuminous; with granular tube casts in abundance; and owes its red color, not to the presence of blood and the red corpuscles, but to the blood pigments, and especially to hæmoglobin.

18. That the more obvious post-mortem appearances in hæmorrhagic malarial fever are substantially the same as are found in other malarial diseases; namely: Liver enlarged and congested, and of dark bronze or slate color; bile abundant in gall bladder and alimentary canal; spleen enlarged and congested; but with the additional pathological complication of enlarged and congested kidneys.

MISCELLANEOUS MEMORANDA.

Mortality.—My collection of cases numbers six-hundred and forty-two (642)—four hundred and eighty-four (484) recoveries, and one hundred and fifty-eight (158) deaths. The percentage of deaths to cases, therefore, is 24.60; or, in round numbers, one-fourth of the cases died, and three-fourths of them recovered.

Black Vomit.—I took a great deal of trouble to secure full accounts of black-vomit cases; but out of the six hundred and forty-two cases reported, only fourteen presented this symptom, and in only three cases is the black vomit reported to have resembled coffee grounds.

It is a curious fact, too, that those of my correspondents who have practised in the most intensely malarial sections of the state, and who have seen the largest numbers of cases of the most malignant types, have not seen black-vomit cases at all; and some of them distinctly take the position that the so-called black vomit of hæmorrhagic malarial fever is always altered bile, and not, like the black vomit of yellow fever, altered blood. Bile is very abundant in this form of fever, and bilious vomiting an almost constant symptom. So that the most probable presumption is that in the vomit of this fever, bile may always, or almost always, be found. Still there may be, by possibility, a few cases in which at some of the later stages of their progress, we may have black vomit containing no bile—a black vomit identical, or very nearly allied to that of yellow fever. Thorough examination—chemical, microscopic, and spectroscopic—of the vomit of this fever is greatly to be desired.

The Specific Poison.—It is the common opinion of physicians familiar with hæmorrhagic malarial fever that it is a true malarial disease, induced by the same morbid poison that gives rise to our ordinary intermittents and remittents. But there are a few who incline to the belief that it is produced by a poison *sui generis*, allied perhaps, to the ordinary malarial poison, but not specifically the same. One of my correspondents especially, namely, Dr. S. M. Hogan, of Union Springs, insists that hæmorrhagic malarial fever is a specific fever, separate and distinct from other malarial maladies, and that it often presents itself in mild cases without the red urine.

In a circular issued in 1881, by the Alabama State Board of Health, I touched upon this conception, as the following passage will show:

"It is now beginning to be generally held that malarial diseases are caused by the introduction into the system of microscopic vegetable organisms—the spores or reproductive germs of certain algæ or fungi. The two researches which have contributed most toward bringing this doctrine into favorable notice, are: 1. The researches of Lanzi and Tesrigi in 1870–76; 2. The researches of Klebs and Tomassi-Crudeli in 1879.

Lanzi and Tesrigi, while ascribing the production of malarial fevers to algæ spores, were not led by their observations to conclude that

the malarial infection is the product of any single species; but rather that algæ spores generally when sufficiently small to pass through the capillary vessels, might all act in much the same way.

Klebs and Crudeli, on the other hand, sought to identify the malarial poison with the micrococci of a single species of bacillus, named by them the *bacillus malariæ*. If their speculations should turn out to be the truth, and the whole truth, it then follows that all forms of malarial maladies are modifications of one common type, and that so far as specific treatment is concerned they must all respond in the same way to the same remedies; while if the speculations of Lanzi and Tesrigi are more in accordance with the facts, it would then seem pertinent to inquire whether the spores of different species of microphytes may give rise to separate varieties of malarial fevers."

The Red Urine.—Strange to say, there has been a good deal of controversy as to whether the red color of the urine in this fever is or is not due to the presence of blood. I consider that Dr. Sternberg's researches have definitely settled this question; and that the fact, so long ago established by Beranger-Ferraud for the African fever, studied by him—the fact, namely, that the red or dark discoloration of the urine is due to the presence, not of blood, but of the blood pigments, and especially of hæmoglobin—is also fully established for the hæmorrhagic malarial fever of Alabama. I am quite willing to believe that the red blood-corpuscles have been found sometimes in this urine; but I feel well assured that it has been only an occasional and, perhaps, accidental phenomenon; and that in all cases the characteristic coloring agent is hæmoglobin. This demonstration would also seem to make it quite certain that our hæmorrhagic malarial fever is identical with the *fièvre bilieuse-melanurique* of Béranger-Ferraud, and other French writers.

The Disintegration of the Blood Corpuscles.—I think there can be no doubt that the disintegration of the blood-corpuscles and the liberation of the hæmoglobin takes place within the blood vessels in the current of the circulation; and that in relation to this pigment, and in relation also to the albumen of the dead corpuscles, the kidneys act simply as organs of elimination.

In the study of this problem the fact mentioned by Dr. Sternberg becomes suggestive—the fact, namely, that the separation of hæmoglobin occurs when bile is injected into the blood current in dogs. Free hæmoglobin has also been found in the blood of patients suffering from malarial hæmaturia, so-called, whatever disease these words may have been intended to designate. But the sufficient demonstration is found in the fact that when blisters are drawn in severe cases of this disease, they yield serum colored exactly like the urine.

NOTES ON TREATMENT.

Under this head I propose simply to offer a few suggestions as to some of the more obvious indications of treatment, based on the pathological characters of the disease, and upon the experience of my correspondents.

1. There is virtual unanimity of opinion as to the superabundance of bile, the congestion of the liver, the engorgement of the portal circle, and the constipation; and there is also virtual unanimity of opinion as to the beneficial influence of purgatives, especially of mercurial purgatives. The methods of administration vary. Some give small doses of calomel, or of calomel in some of the usual combinations of it with other drugs, every few hours, until the bowels are freely moved and the portal and hepatic congestions relieved. Others give larger doses at less frequent intervals; and a few advocate the administration of enormous doses, thirty, forty, and even seventy grains at a time.

2. The disease seems to be always attended by very distressing nausea, and by copious, frequent, and protracted bilious vomiting. It is of course impossible to arrest nausea and emesis as long as there is bile in the stomach. My correspondents say very little about the management of these symptoms, except as to what is indirectly accomplished through the influence of purgation. The indication, however, seems to be very clear that the stomach should be emptied of all the bile that finds its way into it through the duodenum, or in any other way. It seems hardly necessary to administer emetic medicines; but copious drafts of hot water or warm teas will probably be found useful.

3. A dry, harsh skin is another common symptom, mentioned again and again by my correspondents; and in such cases sweating is mentioned as always giving more or less relief. This may be induced by the external application of heat by any of the usual methods; and will be favored by the ingestion of hot drinks as just mentioned in connection with another indication. Very little mention is made of the use of diaphoretic drugs.

4. It does not seem to be settled whether or not it is desirable to address remedies directly to the kidneys. In the meantime all attempts to check the discharge of the red urine by the agency of astringents and styptics have proved certainly unavailing and perhaps mischievous; and the same unfavorable verdict may be confidently pronounced on all attempts to restore through the stimulus of diuretics the action of the congested kidneys when suppression supervenes. The chief reliance for the relief of the overburdened kidneys must be sought in the derivative and compensatory action of the bowels, and especially of the skin.

5. One of my medical friends has found much benefit to result from the free use of oranges and lemons, of which he allows the juice to be ingested *ad libitum*. He is confident that these agreeable vegetable acids exert more effectual control over the stomachic troubles than any other remedies he has ever employed. He also be-

lieves that they act well on the kidneys and the skin; and perhaps also on the blood.

6. As this disease has been almost universally regarded as of malarial origin, it was very natural that our physicians should have looked to quinine as the very sheet anchor in the treatment of it, and so for a long time the general rule was to give quinine in liberal and frequent doses; and with many physicians this is the rule still. It was, however, very soon observed that quinine exercised far less control over this disease than over our ordinary intermittent and remittent fevers. It was also observed that it often increased the flow of red urine, and even reëstablished it after it had ceased to show itself. Strangest of all, it was observed that in many cases the chills came on while the patients were thoroughly under the influence of quinine.

The result of this has been that the confidence of the medical profession of the state in the beneficent power of quinine in this fever has been very much weakened, and that many of them have ceased to use it at all, unless, perhaps, as a tonic in convalescence. Efforts have been made to show the inadvisability of the quinine treatment, by appeals to statistical tables comparing deaths and recoveries with and without quinine. I do not regard these tables as at all conclusive, because of the small number of cases placed in comparison, and for other reasons that need not now be mentioned. In the meantime the broad fact still remains that in the treatment of this most dangerous of malarial maladies, quinine, the great malarial remedy, has lost ground, and is still losing ground in the confidence of the profession. Those who still believe in the efficacy of quinine in the treatment of this malady ascribe the waning faith of their doubting confrères to improper methods of administration. It should not be given, they say, until the system of the patient has been prepared for its reception — that is to say, until the superabundant bile has been purged away, and the cutaneous functions reëstablished.

7. One very remarkable case is related by Dr. R. S. Williams, of Mount Meigs. His son, aged 18 years, had been spending the summer of 1881 on a plantation on the Tallapoosa River, where he had frequent attacks of intermittent fever, which were temporarily controlled by quinine. On the 1st of August he had an attack of intermittent hæmaturia, which also yielded to quinine. He now removed to Mount Meigs, a village free from malarial diseases, and was kept on quinine and iron for three weeks. On the 10th of September, while fully under the influence of quinine, he had a chill, with copious excretion of red urine. This was followed by daily paroxysms for five or six days, the red urine declining gradually, and disappearing completely on the fourth day. On the seventh day he was convalescent, and was placed on Fowler's solution of arsenic, which he continued to take for three weeks. Just twenty-eight days from the previous outbreak he had another attack, commencing with a severe chill, complete prostration, and abundant red urine. The paroxysms returned every day. This time he took no quinine. The urine continued red for several days, and gradually grew less in quantity until it reached about eight ounces in twenty-four hours, when the red color disappeared; it then dropped to four ounces in twenty-four hours, and uræmic symptoms developed; then to one ounce and a half in twelve hours, and then there was complete suppression. During this time he was treated with atropia. Death now seemed inevitable. The pulse was down to twenty, respiration seven, pupils widely dilated, coma profound. In this emergency pilocarpin was given hypodermically — size of dose not mentioned. In less than five minutes a profuse sweat covered the patient's whole body, and there was a copious secretion from the nose. In four hours there was a large discharge from the bowels, and in eight hours a discharge of urine, the first in forty-eight hours, with entire relief to the brain, to the circulation, and to the respiration. The excretion of urine gradually increased until it reached the enormous quantity of 148 ounces in twenty-four hours; and this hyper-excretion continued for eight or ten days, gradually lapsing back to the normal standard.

DR. M. K. TAYLOR said that when stationed in north Alabama he saw some cases like these described, and observed micrococci in the blood of every case before and after death. Blood and albumen in the urine were also frequent. We do not know what the disease is; we call it malaria.

As to treatment, quinine is utterly valueless. In small or large doses it did no good; while it controlled the temperature, it did not control the discharge of blood. Calomel and soda gave the best results, combined with bathing of the skin.

THE STILLBORN.

BY FRED. HUMBERT, M.D., F.C.S.,
OF ALTON, ILLINOIS.

Reading the debate on the etiology of stillborn in the JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, Vol. IV, No. 14, reported from the New York County Medical Society, in which some eminent professors participated, has reminded me of an intention to give my opinion regarding the etiology of stillbirth gathered from experience and observation, not found in obstetrical works, nor in the report above mentioned.

What are the causes that destroy the apparently healthy and fully developed fœtus of, so far as we can see, healthy parents ten or fourteen days before labor sets in? The cases which have come under my observation during fifty-five years of general practice have led me to leave the old travelled path of reasoning from the border line of probability, and begin to view the subject from the period of the dying of the fœtus in the

womb. This led me to find the true cause, and to give such advice as would enable the unfortunate woman to bring forth a living child.

Before entering upon a discussion of this important subject, let us take a retrospective view of the present knowledge of the profession regarding it, so that those who have never met it may be prepared to face it at any time.

From the United States census of 1870 there were 5,282 stillbirths. The mortality report concerning the stillborn is omitted in the census of 1880. I present some reports as gathered from different medical journals. In the city of Baltimore the statistics for 1840 show a population of 102,313, deaths 1,775, stillborn 192, a percentage of 10.9. In the same city, in 1849, there is a population of 169,054, deaths 4,893, stillborn 411, per cent. 8.4. In Mayence on the Rhine, in 1856, there is reported 7,739 births and 464 stillborn. Of these (all I found thus recorded), 236 died during labor, and 228 were dead when labor began, a per cent. of 5.9.

In Philadelphia, in 1858, a population of 575,000 gives deaths 10,895, and stillborn 557, a percentage of 5.7.

St. Louis, in 1873, with a population of 450,000, had 8,551 deaths, and 514 stillborn, a per cent. of 6. In 1874, the mortality in St. Louis was 6,527, and stillborn 474, a percentage of 7.2, but it proved later that the census of 1870 was not correct, which, when reduced, raised their percentage of stillborn above all others to 10. The number of deaths registered since January 1, 1882, is 5,331. This includes stillborn, averaging about ten per week — a percentage of 10½.

Bermuda, in 1872, in a population of 12,785, had 394 deaths, and 14 stillbirths, or 3.5 per cent.

W. M. Findley, M.D., Altoona, Pa., in 300 deliveries reports five cases of stillbirth, 1.7 per cent. This favorable report was from a country practice, and strongly proves that high living—a life of pleasure, without the domestic virtues, greatly increases the number of stillbirths.

Various causes are claimed to produce stillbirth. Let me separate the so long stereotyped reasons of our works on obstetrics, and show more plainly the true cause of a stillbirth after once taking place, and thus be able to prevent another.

Scrofula and syphilis are said to act the most important part as the causes of stillbirth, and of these syphilis is much the more prominent, in its various modifications of tumors, placental degenerations, tubercle, etc. The stage of development of this disease, as first, second, or third, has its peculiar influence upon the offspring.

Syphilis is of slow growth after the healing of the primary ulcer. It lies dormant years and years, no external symptom indicating a body infected by this poison—like a tempting and apparently luscious apple whose inside and core are rotten. If the disease be thus masked in the parent, why not as much, or more so, in the offspring of such a parent? As proof of the influence of the stage of the disease in the parent affecting his child, I give the following cases which I for years have noted.

A merchant, married, on his way to New York, in 1837, to buy goods, contracted a chancre while in Cincinnati. As travel in those days by water and land was slow, he did not return home till healed. Nine months after his return I delivered his wife of a large, full-grown child covered from head to foot with syphilitic scabs, and it died in the next twenty-four hours. The first child of their marriage, a girl of three years, was then living. The lady was in perfect health through her pregnancy, remaining so for thirty years, when she died of pneumonia.

A year and a half after the birth of the syphilitic child, I delivered her of a healthy girl and in eighteen months of another healthy girl.

At this time the husband died of syphilis. In due time she married again — a mechanic, by whom she had one girl. The first-born, before the husband had contracted syphilis, is now living and the mother of eight or nine healthy children. The second girl, born after syphilis developed, was well till her eighteenth year, when, contracting a slight cold, she died of consumption. A year afterward, the last-born from her first husband died of consumption. The child by the second husband is alive and the mother of several children.

Let us divide the case into two parts. First, that in which the death of the fœtus takes place during labor, and second, in which it is evidently dead before labor begins.

The causes for the first are well known, and some of them can be met by the attending physician. One cause not referred to often is presented by Dr. Janeway, in the above-named New York County Medical Society: the improper use of ergot by incompetent midwives. He also states that about one-third of the confinements in New York city are in the care of midwives. In this section of the country, as early as 1833, I observed that all confinements were in the hands of midwives and a few undergraduated (preceptor-taught) physicians. Nearly all cases were normal. The midwives had no knowledge of ergot, and the natural doctor rarely, if ever, used the powder of ergot. During the first twenty-five years here I can recall but two cases of complicated delivery. But since the close of the war deliveries have fallen into the hands of physicians who flood the country and have a hard struggle for existence. I cannot recall more than two midwives in this region. But I have seen more complicated deliveries and death of primiparæ from injudicious use of ergotine and meddlesome practice from graduates of our overnumerous colleges than in those earlier days.

The second class of stillbirths in which the fœtus dies before labor begins, are largely the result of scrofula and syphilis, especially the latter.

There are two classes of these last in which

death takes place before labor. The first, in which disease of the placenta and cord, etc., are the cause. The second, in which the fœtus is full-grown, and no cause can be detected. All that can be seen is the maceration which must have taken place during the last two weeks, since the mother has been conscious of a feeble movement, or of none at all. These women are generally hearty, well built, married, and anxious to have children, and not at all like the class whose only aim in marriage is lust, hating the confinement of raising children. The latter fill the roll of miscarriages.

If the former have a stillbirth they will listen to any advice to prevent its recurrence.

It always was a mystery to me how these fœti must all apparently die within about two weeks of full term in these healthy women. How is it possible that a woman can have five, or eight, or even thirteen stillbirths in succession, the death always occurring ten or fourteen days before full term and delivery. Can syphilis or any other germ lie invariably all this time, and always this definite time, latent; or will it not show modifications in time and potency on the offspring, whether affected by the father or mother? We have seen in the above illustration the effect of a contracted syphilis upon the first, second, and third-born, after the father's contagion. The early authors on obstetrics are silent regarding stillborn. Cazeaux leaves us without any help in our trouble. He says: "It must, however, be confessed that it is often impossible to discriminate the cause of death of the fœtus, or discover anything which can in a satisfactory manner account for it. Some of these unknown causes have attracted attention by the persistency with which they continue to act in the same woman through several successive pregnancies. I myself know a woman, in good health, who on thirteen occasions, without any discoverable reason, lost her child during the last month of gestation." The writer is acquainted with a gentleman of wealth and high social station in our city, whose wife has been delivered successively of seven stillborn children.

Dr. P. Dubois says: "In understanding the fatal termination in preceding pregnancies there is always cause to hope for a happier issue in respect to the one in charge, *so that it is impossible to establish a general rule in reference to matters.*" (The italics are mine.)

How important this matter is when we see in our own incomplete United States census for 1870 reported 5,282 stillborn in one year. In the reports from the cities above how large the percentage, except in Bermuda! Only Dr. Finley, of Altoona, Pa., gives us anything like a complete report. His percentage is light.

Mayence in Germany gives us the most correct because the most specific census, and her average of stillborn before labor began was fifty per cent. The only remedy proposed and used several times to correct this persistency of stillbirth is premature delivery. Denman and several others thought that by bringing on labor before the period at which the foetus perished in preceding pregnancies there would be a chance of obtaining living children. This Dr. Tyler Smith carried out. He reports that the amount of prejudice will be placed on its proper footing. I saw a year or two ago a case with Dr. Traucer in which premature labor was brought on after the mother had given birth to several dead children in succession. On this occasion a living child was born. But in the next pregnancy the patient declined the operation and another stillbirth occurred. Such premature delivery was advocated by the president of the New York County Medical Society in the above-mentioned debate. He had successfully delivered a patient of a living child by the induction of premature labor. I think I have shown plainly in the above review of the history of stillbirth that there is a mysterious cause for such cases as die before labor begins. By reasoning from the following case I think I prove to have found the true and only cause of death in most of these cases.

In 1867, month of January, a farmer requested me to attend his wife during confinement.

I delivered her of a normally developed stillborn child. Indications from maceration of the skin were that it had been dead about two weeks. After examining the placenta and cord, which were normal, I asked the husband, a stalwart man of five feet ten inches, with curly, light hair and blue eyes, and forty years of age, about his previous married life. He said this was his second wife, and she was a widow with two living children when he married her. His first wife died in childbirth, and was delivered of a stillborn. Every child by this wife, this the fifth, was stillborn. As I looked at the husband, with his liquid blue eyes and sensual physiognomy, and at the wife, a strongly built, obese brunette, with sexuality marked, the thought struck me that they must have indulged in copulation during and to the end of pregnancy. Toward the end of gestation the fruit-water is mostly absorbed, and the foetus, lying close to the wall of the womb, is not so well protected as in previous months.

I concluded the spasmodic contraction of the uterus on such occasions, with the paralyzing effects of coitus, either directly killed the fœtus or gradually, by repeated acts, produced its death. So strongly was I impressed with this reasoning that I went to the patient and told her if she should be pregnant again she should have no connection with her husband for at least four months preceding the end of her term. October 22, 1868, I delivered her of a living boy, and on December 22, 1870, I delivered her of a living girl; October 27, 1872, she had a miscarriage. These are all the pregnancies she has had, and she is now past the period of gestation. Both children are living and healthy. Her boy and girl by her first husband are alive and have fam-

ilies. Both parents are to-day in good health. In this case, after five successive stillbirths, a living boy was born, thus proving that my advice when first attending her was good, and more was I assured of this when, afterwards, a living girl was produced.

After this happy result my *tabula rasa* brought slowly back to me a remark of my professor in Vienna in 1830, in one of his lectures. He said Sophia, the Archduchess of Austria, a Bavarian princess and prominant political leader at court, being pregnant, was advised by her physician to leave the city and live at an imperial manor in Bohemia, away from all excitement, that she might secure a living child. I do not know that she had a stillbirth or miscarriage before. But a child in this family was then of great importance, as the Crown Prince was without children. There she was delivered of a living boy. In 1848, Sophia (then the leader in the counter revolution growing out of the resignation of Ferdinand) made that boy, then 18 years of age, his successor. He is now the emperor of Austria and Hungary.

Had not this nearly effaced fact in my memory assisted me to the present conception and advice, as all original ideas are generally developed from previous undeveloped thought?

Alton, Ill., May 8, 1885.

MEDICAL PROGRESS.

MEDICINE.

Inoculation of Tuberculosis in a Young Girl.—E. A. Tscherning describes the case of a young woman twenty-four years of age, who was a cook in the house of Prof. Holmes, who died of phthisis florida, in six months after its development. She had been healthy and robust, with no evidences of scrofulosis or tuberculosis. In the last days of the life of the professor his expectoration was almost a pure culture of the bacillus in pus; two days before his death, the cook pricked herself on the side of the median finger of the left hand with a pointed piece of broken glass, which came from a spittoon. Fourteen days after the accident symptoms of paronychia showed themselves. Phenic compresses relieved the symptoms somewhat at the end of eight days, but there was no suppuration. Circumscribed hardness of the size of a pea was felt in the subcutaneous tissue. On the following week there was also œdema and slight pain. M. Tscherning incised and removed the tumor; it was composed of granulations, and situated between the skin and the tendon. The wound healed by first intention. This was at the end of August.

At the beginning of October, the patient complained of pains on flexing the fingers. The skin and subcutaneous tissue were tumefied over the phalanx and in the palm of the hand. No tenderness over the tendon. In November, a thickening of the sheath of the flexor tendon could be felt. The functions of the finger were interfered with, and there was a little pain; at the same time the two cubital and two axillary ganglia of the same arm were found to be tumefied. In other respects she was very well. November 21, Prof. Studsgaard extirpated the cubital and axillary ganglia, disarticulated the medius at its metacarpo-phalangeal articulation, incised the palm of the hand, and extirpated the tumefied tendon and its sheath. He excised and scraped the subcutaneous granulations. Sublimate dressings. Reunion on the eleventh day.

The pathological changes were the following: the sheath of the tendon was filled with pale granulations, the serous investment of the tendon was covered by petechial patches. No pus or grumous matter; no alterations in the bone or articulation. The granulations, when examined with the microscope, showed a quantity of elementary tubercles, many of which presented a caseous degeneration in their centre; there was a number of large cellules and giant cellules. The extirpated ganglia appeared to the naked eye to be simply adenitis without pus. The microscope showed a hyperplasia of large cells, with tuberculous granulations. In all the sections of the ganglia or tendinous sheath, there were found, by using Ehrlich's method, the tuberculous bacilli very markedly shown, isolated, and sometimes in groups of two or three, in the form of a more or less open V. Often they were accompanied by what are called spores.—*Revue Bibl. Univer. des Sciences Médicales*, Jan. 31, 1885.

Area Celsi.—That alopecia areata depends upon the growth of a fungus is a doctrine that has been often advanced only to be refuted, and has now few, if any, supporters. Nor can there be any doubt that mistakes have been made in the various attempts to settle what might seem to be a simple question, and certainly, among those who have upheld the parasitic nature of the affection, there has been little agreement as to the precise nature of the fungus concerned. It has been reserved for these days of microphytic pathology to revive the doctrine in a more refined form. Alopecia areata is due to the growth of a micrococcus, at least so says Dr. von Schleu, of Munich, in a paper contributed to the current number of *Virchow's Archiv*. He describes five cases, in all of which the stunted and diseased hairs were mostly the seat of colonies of micrococci, averaging 0.5 m. in diameter, some smaller ones being of the diplococcus form, and larger ones with median constriction. From their situation in the shaft, it would seem that they occurred in connection with the ducts of the sebaceous glands, and it is inferred that they invade the hairs by these ducts. The micro-organisms were not found in any but the diseased hairs, and not universally in them. By cultures in agar-agar peptone it was ascertained that the organisms were distinctive in their mode of growth, and experiments were made with a view to ascertain

whether they were really capable of transmitting the disease; on the human subject the attempt failed, but on white rats there appeared, on the spots where the material of some of the cultures had been rubbed in, scaly patches, over which the hairs at first became yellow-colored, and then fell out. The writer reconciles the notion that these micro-organisms have a causal relation to the affection, with the other well proved facts of its association with trophic disturbances (*e.g.*, vaso-motor derangements), by assuming that the occurrence of such disturbance prepares the soil for the growth of the micrococcus. Holding such views, he advocates, and has had good results from, epilation and the use of corrosive sublimate; and also urges the importance of cleanliness in the use of brushes, etc., as a prophylactic measure.—*The Lancet*, March 28, 1885.

A LITTLE-KNOWN SYMPTOM OF MENINGITIS.—It is well known that most cases of tubercular and epidemic cerebro-spinal meningitis exhibit the classical symptoms of rigidity of the neck and back, which on the other hand does not characterize cases of purulent secondary meningitis. In nearly all these cases, under ordinary circumstances, the extremities remain supple, but contractures have been observed in a few. If, however, a patient with meningitis who presents this dorsal stiffness be raised up and made to sit on the edge of the bed, the rigidity becomes much more intense, and, what has hitherto escaped notice, contracture in flexion sets in at the knees, and sometimes at the elbows also. This symptom seems to depend on the flexion of the thigh on the trunk proper to sitting. If flexion is brought about while the patient is lying down, the contracture sets in, while the contracture vanishes when the patient stands upright. But it cannot while the patient is reclining, be provoked by mere compression of the sciatic nerves. Contracture of the limbs, like that of the back and neck, characterizes the whole course of the disease; it outlasts the fever, and is demonstrable in advanced convalescence. It has nothing in common with that rigidity of the back which may be induced in very old or very young patients who are suffering from some feverishness or from alcoholism. This, which comes on when the attempt is made to raise the patient in bed, is nothing but an instinctive resistance to movement of uncertain occurrence.

Since 1880 Kernig has had the opportunity of observing fifteen cases of undoubted meningitis, viz.: thirteen of infectious cerebro-spinal meningitis, one of tubercular and one of purulent cerebral meningitis with chronic parenchymatous nephritis. In every case the new symptom was noticed. In all other diseases, where the symptom was sought, it was absent, except in six cases of doubtful diagnosis, in which contracture of the legs occurred when the patient sat upright. In five of these, some serious affection of the cerebral or spinal meninges was discovered *post mortem*—œdema, pachymeningitis hæmorrhagica, thromboses of sinuses—and even in the sixth there was hyperæmia of the pia mater.—*Berlin. klin. Wochenschrift*, Dec. 9, 1884; *Cincinnati Lancet and Clinic*, Feb. 28, 1885.

WHITE OF EGG IN TREATMENT OF DIARRHŒA.—Celli recommends this remedy in chronic diarrhœa especially if phthisical. The whites of eight or ten eggs are beaten up in water Oj, so as to form an emulsion; a little laudanum and a little flavoring extract added, and the whole taken in 24 hours.

SURGERY.

ANEURISM OF THE POPLITEAL ARTERY.—DR. W. H. CARROLL, of Paterson, reports the following case occurring in the service of Dr. C. S. Van Riper, at St. Joseph's Hospital, in that city. The patient, a laborer, had noticed, for three months prior to admission to hospital, a swelling on the inner and posterior surface of the thigh, which was quite painful when the limb was flexed. On examination a small oval tumor, about the size of a pigeon's egg, was found lying a little to the inner side of the popliteal space. A distinct heaving impulse was felt on palpation, and on auscultation a bruit was perceptible. Several exostoses were felt on different parts of the limbs. A diagnosis was made of aneurism of one of the recurrent or lateral branches of the popliteal artery. Twelve days after admission the patient, while sitting in a chair, experienced suddenly a severe pain in the tumor, which became greatly enlarged and inflamed. Dr. Van Riper, who saw the case two days later, pronounced it one of rupture of the aneurismal sac, and proposed operation. On the following morning, the patient being etherized, an incision, six inches in length, was made through the skin and subcutaneous tissue. The muscles being retracted, the sac was exposed, and was found filled with a quantity of dark, clotted blood. This was turned out and the sac was thoroughly washed with a two and a half per cent. solution of carbolic acid. It was found that the rupture had been caused by the pressure of an exostosis situated near the inner bifurcation of the linea aspera. This was removed by the bone-forceps, and the femoral artery was then ligated at the lower end of the Hunterian canal and at the lower boundary of the popliteal space. The Esmarch bandage being then removed, it was found that a small opening existed in the popliteal vein. An attempt was made to ligate the vessel, but its coats were so soft that the ligatures would not hold. The vein, and as much as possible of the surrounding tissues, were then seized by the clamp-forceps and the latter were left *in situ*. The edges of the wound were then approximated by silk sutures, and dressed with iodoform and absorbent cotton. The patient was somewhat exhausted, but rallied under the influence of stimulants. On the sixth day the clamp was removed. The wound healed kindly, and the patient made a good recovery.—*The Medical Record*, April 4, 1885.

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THE EDITOR of this JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information, should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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ACTION OF THE AMERICAN MEDICAL ASSOCIATION REGARDING THE ARRANGEMENTS FOR THE INTERNATIONAL CONGRESS OF 1887.

In discussing this subject we have but one object in view, namely, to make plain, if possible, the present condition of the work of organization, and the best mode of carrying it forward to a successful result. Consequently we shall make as few personal allusions as possible, and assume that all parties have been actuated by good motives, and an earnest desire to make the International Congress to be held in the capital of our own country in 1887 fully equal in all the elements of success to any of the meetings that have preceded it.

The leading facts are, that at the meeting of the American Medical Association in Washington, in May, 1884, the President, in his address, called the attention of the Association to the propriety of extending an invitation to the Congress then soon to assemble in Copenhagen, to hold their next meeting in this country. That part of his address was referred to a special committee, which, at a subsequent stage of the proceedings, submitted the following report:

Resolved, 1. That a committee of seven, of which Dr. Austin Flint, the President of the Association, shall be a member, shall be appointed by the President.

2. It shall be the duty of this committee to extend, in behalf of the medical profession of the United States to the International Medical Congress, about to meet in Copenhagen, a cordial invitation to have the next International Medical Congress meet at Washington, D. C., in 1887.

Resolved, 1. That the committee shall elect its own officers, and that, in case the invitation is accepted, it shall proceed to act as an Executive Committee, with full power to fix the time and to make all necessary and suitable arrangements for the meeting of such Congress, and to solicit funds for that purpose.

2. That the committee shall have power to add to its membership, to perfect its organization, and that to meet the preliminary expenses of printing, postage, etc., the chairman of this committee is authorized to draw upon the treasurer of this Association for an amount not exceeding four hundred dollars.

This report was adopted without opposition or discussion, and the President subsequently announced the proposed committee as follows: Austin Flint, Sr., of New York; I. Minis Hays, of Philadelphia; L. A. Sayre, of New York; C. Johnson, of Baltimore; Geo. Engelmann, of St. Louis; J. M. Browne, of U.S.N., and J. S. Billings, of U.S.A.; to whom H. F. Campbell, of Augusta, Ga., then President-elect of the Association, was added by vote of the Association, making the committee consist of eight members.

The committee thus constituted, proceeded to Copenhagen, extended the invitation according to instructions, and it was accepted. In accordance with the foregoing resolutions, the committee, soon after the return of its members, selected fifteen or twenty prominent members of the profession to constitute additional members of the committee. The committee, thus enlarged, was requested to meet in full session in the city of Washington on the 29th of November, 1884, to commence the preliminary work for organizing the proposed Congress.

At the time appointed a majority of the members of the enlarged committee were present, and entered earnestly upon a consideration of the work committed to them. A sub-committee of the original committee of eight had outlined a plan of organization and a system of rules, in part, at least, selected from those adopted by previous Congresses in Europe.

This plan made no provision for representation by delegates from any medical society organizations, either state or national, but contemplated the selection of officers for the Sections, and important committees, solely on account of their reputation at home and abroad, without regard to their membership in medical societies or the sections of the country in which they lived, while the American part of the membership of the Congress would consist of such parties as the Executive Committee should invite. After a pretty free discussion the essential fea-

tures of this plan were set aside by the adoption of resolutions providing for a full and uniform representation by delegates from all the regular and legitimate society organizations, national, state, and local, and a strong effort was made to have the selection of officers of Sections so distributed as to fairly represent the whole of our country. The latter, however, was not as successful as we desired. The time of the meeting of the general committee was necessarily limited, and a large part of the details had to be left with an Executive Committee of five, whose work was subject to the approval of the general committee. After adopting the general rules, as since published, and filling some of the offices, the general committee adjourned, subject to the call of the Executive Committee, but with the full expectation of meeting again in New Orleans during the meeting of the Association, if not sooner called together by the Executive Committee. The latter committee, however, regarded their work so far advanced as to render a meeting of the general committee in New Orleans unnecessary, and proceeded to the publication of their work as far as completed. This, we think, was an unwise step. We think the general committee should have assembled in New Orleans and reported its action to the Association before its formal publication to the world. It would have afforded an opportunity for conferences and adjustment of differences at once, and would have avoided the charge of having ignored the body from which its existence and all its powers had been derived. If an error, however, it was certainly one of judgment and not of design. Regarding the power conferred upon the committee by the resolutions we have quoted, as ample and unreserved, the members were simply intent on the early and efficient discharge of the duties imposed on them, without unnecessary expenditure of time and money.

The idea that the members of the committee having charge of the work of organizing the International Congress had acted from any other motives than an honest desire to execute the trust committed to them to the best of their ability, is without the slightest foundation, and should be discarded by every honorable mind. But it is plainly evident both from the expressions in a large part of the medical press, and from the sentiments freely expressed in private conversation, as well as publicly, that two important errors had been committed in the work of the committee. The first was committed by the original committee of eight members appointed at Washington, in the selection of additional members of their own body. Actuated, perhaps, by an injudicious liberality, it is claimed that they included in their selection some who had placed themselves in antagonism to the national and state organizations of the profession, by openly repudiating the national Code of Ethics, which constitutes the common bond of union for all these organizations.

By this step, they placed the American Medical Association, from which the committee had derived all its power, in an inconsistent position, and failed to sustain the large majority of the profession in the state of New York who had faithfully sustained the national code, and maintained their fraternal relations to the national and state organization of the whole country.

The second alleged error consisted simply in a failure on the part of the enlarged committee to appreciate the importance of so distributing the officers of Sections as to represent and interest, as far as possible, the members of the profession in all the leading geographical divisions of our country.

That the chief object of the Association in its action in New Orleans was to correct these alleged errors, is evident from the character of the action taken, which consisted solely in the adoption of the two following resolutions:

Resolved, That the committee appointed by this Association to arrange for the meeting of the International Medical Congress in America, in 1887, be enlarged by the addition of thirty-eight members, one from each state and territory, the army, navy, and marine hospital service, to be appointed by the chairman at this meeting, and that the committee thus enlarged shall proceed at once to review, alter, and amend the motions of the present committee as it may deem best.

This was adopted during the general session of the second day, when the whole subject was under discussion. The second resolution was proposed and adopted almost immediately after the opening of the general session on Friday morning, the last day of the meeting, and is as follows:

Resolved, That the committee appointed in pursuance of a resolution adopted by this Association, April 29, 1885, to constitute an addition to the original committee of seven, previously appointed to invite and make arrangements for the meeting of the International Medical Congress to be held in Washington, D. C., in 1887, be, and the said committee is, hereby authorized and empowered to select a chairman and secretary, and to fill all vacancies that may

occur by death or inability to attend the committee meetings, and to appoint the officers of the Congress.

A fair and unprejudiced study of these resolutions will show their practical effect to be the elimination from the previously existing committee on the organization of the next International Medical Congress, of all those who had been added by the original committee of eight, appointed at the meeting in Washington, and the substitution of the one from each state, territory, etc., just appointed by the Association in New Orleans, and whose names are given on page 552 of this JOURNAL for May 16, 1885. Whatever doubts might have existed in regard to this point from the language of the first resolution are dispelled by the more explicit designation of the "original committee of seven," in the second.

The committee thus constituted of the eight originally appointed at the meeting in Washington in 1884, and the thirty-eight assumed to have been appointed by the meeting in New Orleans in 1885, according to these resolutions, is now the existing committee on the further organization of the International Medical Congress of 1887, and is authorized to fill any vacancies that may occur, and "review, alter, or amend" any of the acts or work performed by the committee previous to this change.

There is nothing in the resolutions quoted, or in any other action of the Association during the meeting in New Orleans, that directly destroys the previous official organization of the committee, or nullifies a single item of the work already done towards organizing the Congress.

On the contrary, the Association has simply assumed and exercised the right to alter the *personnel* of the committee, by making the additions to the original committee of invitation representatives of each state, instead of selections from a more limited portion of the country. In looking over the list of those selected to represent the profession of the several states, we recognize in nearly all of them men who are well known and in good standing in their respective states; men of sound conservative qualities, who, if promptly supplied with printed circulars containing the regulations already adopted, and invited by the original committee, through their officers, to an early meeting at Washington, or any other suitable place, like Chicago, for example, will be found disposed to make no unnecessary changes in the work already done; and will cordially and earnestly and intelligently work for giving to the Congress the highest degree of success. If there are in the list two or three whose zeal may be largely in excess of their wisdom, they certainly constitute so small a minority that they should be capable of doing little or no mischief. Nothing, however, could give such persons so good an opportunity for making mischief of a serious character, as for the original committee to hesitate in accepting the new order, and to be dilatory in moving forward as the recognized leaders in the further prosecution of the important work before them.

THE EFFECT OF THE SALINE INGREDIENTS OF THE BLOOD UPON THE CONTRACTIONS OF THE HEART.

Such is the title of a paper by DR. SIDNEY RINGER in the *British Medical Journal*, of April 11, 1885. It is very well known that if blood or an artificial circulating fluid be sent through the cavities of a frog's heart, entirely detached from the body, the heart will continue to beat for some hours; and that this is also true of portions of the heart, as the lower third of the ventricle, which has long been supposed to be free from nervous ganglia. "Here, then, we have a means of testing with facility the immediate action of a drug on the whole heart, on the ventricle, or on a portion of the ventricle." He proposes, in this communication, to describe the behavior, "in physiological doses, of the natural salts of the circulation—the salts proper to the blood itself—on the frog's ventricle."

When pure water is made to flow through the ventricle, the condition known as water-rigor soon ensues, and the contractions cease. If common salt be added to distilled water, in the same proportions in which it exists in the blood, the contractions grow more weak, contractility finally ceases, and the ventricle is arrested in diastole; a contraction cannot be now excited by even a strong galvanic induction shock. "The addition of any other of the saline constituents of the blood, save one, will not restore the suspended contractility. The only constituent which will restore the suspended contractility is lime. Spontaneous contractions at once return when the physiological proportion of lime salt is added to the solution. "Lime salts, therefore, will sustain the contractility of the ventricular muscular tissue, and indeed are essential to the maintenance of the contractility. In fact, without a lime salt, no single salt, nor all the saline con-

stituents of the blood combined, can sustain contractility."

But the lime salts, though able to sustain the contractility of the heart, or to provoke it when it has disappeared, cannot maintain the circulation; "for the diminution of the dilatation, owing to the fusion of the beats, by a half or two-thirds, lessens correspondingly the amount of blood the ventricle can receive, and, therefore, propel into the arterial system." He shows that a potassium salt, in physiological quantity, obviates this difficulty, "and insures a perfectly natural contraction." The fusion of the beats is caused by the commencement of a contraction before dilatation is completed; but the addition of the potassium salt accelerates contraction, partially antagonizing the action of the lime salt, and the contraction is natural. "Thus we have produced, by the combination of calcium chloride, potassium chloride, and sodium chloride, a neutral solution capable of sustaining the ventricular contraction, and of producing perfectly normal contractions." But this fluid is neutral in reaction, and though the contractions may continue for a while with a neutral fluid, they finally grow weaker and weaker; it is, therefore, necessary to add that which will alkalinize the fluid—a physiological quantity of sodium bicarbonate. This effect of the sodium bicarbonate is due, the author thinks, to the fact that it neutralizes the acid developed in the contracting ventricle itself; for the addition of a very small quantity of acid to a circulating fluid is sufficient to arrest the contractions.

"The normal contraction of the ventricle, then, is the result of a mutual antagonism between calcium and sodium bicarbonate salts on the one hand, and potassium chloride on the other. We can modify, in various ways, the character of the contraction, by altering the normal relative quantities of these salts. . . . On this physiological antagonism the proper contraction of the heart depends. . . . In conclusion, let me urge a practical suggestion, which seems to spring naturally from these experiments. In cases of profuse hæmorrhage, as in flooding, or in the excessive alvine discharges of cholera, it is of cogent importance that we should employ a fluid suitable to the life-or-death case. . . . The foregoing inquiry points out that an effective injection should contain not only sodium bicarbonate, but likewise physiological quantities of salts of calcium and potassium."

SOCIETY PROCEEDINGS.

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, Thursday, May 7, 1885.

THE PRESIDENT, B. F. BAER, M.D., IN THE CHAIR.

The President read the following report of a cure of

SECONDARY OVARIOTOMY.

and exhibited the specimen, a small papillomatous cyst:

Mrs. S., æt. 33, married first at fifteen years of age; became a widow at twenty and remarried at twenty-six. Had one child only, by the first marriage, seventeen years ago. Enjoyed good health, except that she has always been very nervous, until nine years ago, when she suffered from an inflammation in the pelvic region, which confined her to bed six weeks. Has suffered ever since from profuse and frequent metrorrhagia, with paroxysms of left ovarian pain. About four years ago she also began to suffer from hysterical attacks of an epileptiform character. Two years after these nervous manifestations appeared—in the spring of 1883—she noticed that her abdomen was beginning to swell. The growth continued to increase, and with it the metrorrhagia, pain, and epileptiform seizures. She lost flesh rapidly, becoming weak and bedridden.

In January, 1884, ovariotomy was performed. After her recovery, which was slow, and despaired of for some time, she enjoyed comparatively good health. The menstrual flow became regular and painless and the convulsions ceased entirely. But in the September following she had another attack of uterine hæmorrhage which lasted three weeks. Pain also returned, in character like that attending the development of the first tumor, but it was now located on the right side instead of the left.

In October, she consulted my friend, Dr. D. J. Miller. Pain and hæmorrhage were at this time very severe, and she was weak and anæmic. On examination, Dr. Miller found an abnormal development, in the region of the right broad ligament, about the size of an egg. This was closely adherent to and apparently within the folds of the ligament. It was very tender to the touch, but there were no constitutional evidences of acute inflammation. Shortly after this she had a recurrence of the epileptiform convulsions. Treatment seemed to control the symptoms for a time, but from the latter part of December they increased steadily. The constant drain from the metrorrhagia, the increasing violence and frequency of the paroxysms of pain, and the convulsive attacks began to tell in a marked degree upon her health in loss of flesh and strength. In February, 1885, through the kindness of Dr. Miller, I saw the patient. At that time the growth had attained the size of a large orange; it occupied the right side of the pelvis, displacing the uterus to the left, and seemed to be closely attached to the broad ligament and

the uterus. I agreed with Dr. Miller's diagnosis of ovarian cystic disease, and although the tumor was evidently adherent to the uterus and broad ligament and therefore probably did not have a pedicle, I advised its removal for the relief of the grave symptoms.

Operation, April 7, 1885, at 12 M., assisted by Drs. D. J. Miller, Ed. H. Small, and G. P. Perley, in the presence of Drs. B. C. Miller and Plummer. *Incision*, one-half inch to right of old cicatrix, three inches in length, passing through the body of the right rectus muscle. After clamping all bleeding vessels, I opened the peritoneal cavity and found that the omentum was adherent to the line of union of the first incision, which had been about eight inches in length; and that adhesions also existed between portions of intestine and the location of the former pedicle on the left of the uterus.

The necessity of pushing the omentum and intestine aside to get at the tumor, and fear of breaking the adhesions by so doing, together with the small size and deep-seated location of the cyst, made the required manipulations exceedingly difficult. When I obtained a view of the upper surface of the tumor, it looked not unlike the pregnant uterus in color and vascularity. Its outer wall was interlaced with a network of veins, some of them as large as a quill. Exploration with the fingers showed it to be so deep in the pelvis and so closely attached to the uterus, Fallopian tube, and broad ligament that they seemed to be one mass, the whole attached by a broad surface to the pelvic floor. The prospect of completing the operation was now anything but bright, because of the danger of opening this very vascular wall for the purpose of enucleation. But I began with an attempt to separate adhesions, and after patient, gentle, but persistent efforts, they began to yield and I was finally enabled to get two fingers around the posterior surface of the tumor, without producing much hæmorrhage. I next passed a small trocar of the aspirator and drew off about six ounces of a straw-colored fluid; this gave me a little more room, and I now hoped to be able to remove the tumor entire, and with that purpose in view I endeavored to still further separate it from its attachments; but so much hæmorrhage occurred that I was compelled to desist. I next very carefully broke the outer wall of the cyst with my finger and proceeded to shell out the lining membrane; but it was so friable that it gave way and my finger entered the cavity of the partially collapsed sac. I found it filled with a papillary, cauliflower-like growth. This I removed to prevent its escape into the peritoneal cavity, where I feared it might propagate by contact, if lost and allowed to remain. I then completed the enucleation of the internal surface of the tumor. The thick and vascular external wall was now so thinned and drawn out that it formed a sort of pedicle. This I transfixed and ligatured, cutting away the redundant portion. Then after thoroughly removing all foreign material, I closed the abdominal incision with seven silk sutures and returned the patient to bed.

She recovered promptly from the ether, showing no evidence of shock. At 8 P.M. her temperature was normal and pulse 126. On the evening of the second day her temperature rose to 101° and a free metrorrhagia occurred. The temperature fell within a few hours to 99 2-5° and did not afterward rise above 100°.

On the fourth day I found union so complete and solid that I removed the sutures. She sat up on the nineteenth day and has now entirely recovered from the operation. The result on the hystero-epilepsy and other nervous symptoms will be reported later by Dr. Miller.

It is of vital importance to every woman who must submit to the operation of ovariotomy, that the condition of the opposite ovary should be thoroughly investigated. If found to be diseased, even slightly, I think it should be removed to shield the patient from the danger of a probable second operation. But when such nervous symptoms as existed in this case are present, though the opposite ovary should be found to be healthy, I think it is imperative upon the surgeon to remove it with the hope of relieving these symptoms. It does not add much to the danger of the operation; indeed, in my own practice it would seem to have had the reverse effect, for I have lost but a single case of double ovariotomy, though about one-third of my cases have been of that character.

DR. MONTGOMERY takes exception to the statement of advisability of removal of the other ovary. The second ovary, even if slightly diseased, may be the source of repeated pregnancies. He had, in one case of ovariotomy, left the other ovary, although it was enlarged and contained several small cysts; pregnancies and the delivery of living children occurred subsequently, but there has been no sign of another ovarian tumor. In some cases it might be advisable, but no such fixed rule should be formulated.

DR. GOODELL has performed 144 ovariotomies, and has had secondary operations in two of them in which the remaining ovary was apparently healthy at the time of the primary operation. In all cases after the menopause he now removes the second ovary, even if it seems to be perfectly healthy.

The President was much gratified to have Dr. Goodell's indorsement of his opinion upon the advisability of the removal of the second ovary.

DR. MONTGOMERY reported

AN OÖPHORECTOMY.

Mrs. C., æt. 42, consulted me in November, 1884, with the following history: She is married, but never became pregnant. Menstruated once when fourteen and not again until seventeen, when she became regular, but the flow was always preceded for a few days by severe cramp-like pains which continued during the menstrual period. In her earlier menstrual life the flow was quite free, but

later it has been scanty. The pain was felt in the back and in each inguinal region. She had an attack of small pox during her fourteenth year, but otherwise had enjoyed good health until a few years ago. Two years since she noticed that she was losing flesh, her appetite became poor, she had constant nausea and frequent vomiting, the pain in the back and inguinal regions occurred in the after part of each day, was exceedingly distressing, and interfered with her rest at night. This pain was greatly aggravated by walking, standing, riding in cars, and by coition.

When she came under his care she had lost considerable flesh, had a very irritable stomach, and was not free from pain an entire day at a time. The uterus was retroverted, apparently bound down to a thickened mass posteriorly, and presented a catarrhal endometritis. Pressure upon the fornix vagina gave rise to severe pain. Some efforts were made to raise the uterus by means of tampons of cotton, but they only increased the pain. The patient soon became unable to move about and was confined continuously to bed. The pain was constant during the greater part of the twenty-four hours, so that during the last three months she had had but three nights of uninterrupted sleep, though morphia was given daily. The trouble, from the first, had been ascribed to ovarian disease, and every effort was made to improve her condition preparatory to the removal of the ovaries, but without avail. Her emaciation became extreme, when it was decided to run the risk of the operation. At this time her menses had been absent about two months.

On April 18, assisted by Drs. W. H. and C. B. Warder and Dr. Martin, the ovaries were removed. They were situated in Douglas' cul-de-sac behind and beneath the uterus, and were slightly adherent. Both ovaries were enlarged and hard, and presented a number of small cysts. The latter were found also in the broad ligaments. The wound was closed with silk sutures and dressed with salicylated cotton. Her subsequent comfort was very much interfered with by the pressure from lying, which, in spite of all precautions, produced a small slough over the sacrum. Her highest temperature, 102 degrees, was reached at 6 P.M. on the 20th. The wound healed by first intention. The sutures were removed on the sixth day. After the third day no anodyne was given, but she slept nine hours each night. All her old distressing symptoms vanished as if by magic, her appetite and digestion became good, and she began to improve in general appearance. There has been no recurrence of the pain since the operation.

DR. HENRY BEATES reported a case of

CONDYLOMATA URETHRÆ,

it having closely simulated epithelioma in appearance. As the routine notes will be uninteresting, they will be omitted. Mrs. D., multipara, æt. 29, sought relief from an intense dysuria which was of a little more than a year's duration. A history indicating an attack of gonorrhœa, while not positive, was sufficient to render it probable. A year or two after marriage she was attacked with either acute cystitis, urethritis, or gonorrhœa, which disappeared, but was followed after a lapse of two or three months by frequent micturition and vesical tenesmus. After a few weeks a urethral discharge, purulent in character, was noticed, followed soon after by the development of condylomata on the margin of the meatus urinarius. Coincidently the tenesmus and dysuria increased, and by interference with sleep, and suffering entailed, resulted in impairment of health. Remedies had been prescribed, but no examination of the local condition made. When first seen, the patient was almost constantly harassed with tenesmus, and was suffering intensely. She would retain the urine as long as possible, and void it only when the voluntary control of the bladder was overcome by the vesical contraction. Examination revealed numerous ous condylomata originating from the margin of the meatus and interior of urethra. The meatus and its surrounding neoplasms were bathed in a sanguineo-purulent fluid, and the inferior border and membrane of vestibule were the seat of destructive ulceration. This latter so closely simulated the appearances seen in ulcerating epithelioma that that diagnosis was confirmed by a gentleman who saw the case with me. The external growths were snipped off with scissors and the bleeding bases staunched with nitric acid. After a short preparatory treatment anæsthesia was produced, the urethra dilated, and the condylomata, which were numerous, and derived from the urethral membrane only, removed with the dull curette. A curious point was the distinct demarcation of the morbid condition, which was limited at the vesical terminus of the canal, and the perfect freedom of the vesical membrane from the growths. A solution of chloral, iodine, and carbolic acid was thoroughly applied and had the effect of stopping hæmorrhage. The ulcerated tissue and a portion of the urethral floor were incised and the margins treated with nitric acid. The microscope revealed hypertrophy of the mucosa, with increase of epithelial elements and capillaries. Four years have elapsed, and no evidences of return have presented themselves. I believe the cause of these condylomata to have been a specific urethritis which, becoming chronic, by its irritating discharge determined the growths.

LEIOMYOMA UTERI.

Mrs. M. German, æt. 37, mother of one child, history free from neoplastic predisposition. At thirteen the catamenial function was started and became regular soon. Congestive dysmenorrhœa for a few months at first; once every twenty-eight days, lasting four days. Married at nineteen, conceived two years later, delivery at term after a tedious instrumental labor. About a year later a steadily increasing menorrhagia com-

menced, with leucorrhœa. She was unsuccessfully treated for this for two years, and came under the care of a well known practitioner of Philadelphia, since deceased, who introduced pieces of lunar caustic into the uterine cavity and allowed them to remain there until dissolved. After a few weeks the menorrhagia was relieved, and fair health was enjoyed for more than two years. The hæmorrhage recurring she again visited Philadelphia, and was treated with the dull curette and endometrical applications. She remained free from her trouble for three years, during which time she met with financial reverses and was subject to the depressing influence of trouble. She had never been impregnated since the birth of her only child. Hæmorrhage recurring, she entered a hospital in western Pennsylvania, where she remained two years under unsuccessful treatment. All treatment was abandoned, and the hæmorrhagic condition suffered for nearly five years, when threatening death brought her again to Philadelphia. She attended the Nurses' Home Charity where I saw her, as an assistant in 1879. Her treatment included iodine to the cavity, ergot and gallic acid internally, and the curette. In October of the same year she placed herself under my care. I was called to see her and found her in profound syncope. She had occasionally manifested mental aberration, due entirely to the sanguineous state, and for two or three months was bleeding almost constantly. A laminaria tent was promptly placed, and a thorough examination made, which disclosed the presence of a neoplasm occupying the right lateral and posterior uterine wall. Its contour was regular, base broad, consistence that of uterine tissue. Its submucous location was determined and enucleation effected. The growth was as large as a doubled fist, and required division before it could be delivered. It weighed nine ounces. Profuse bleeding occurred during the operation, and the shock was alarming; reaction, however, occurred. Microscopic examination revealed a typical leiomyoma uteri of the telangiectatic type. When last heard from, a few months ago, she was enjoying excellent health.

Dr. Goodell exhibited specimens of

PAPILLARY OVARIAN CYSTS

removed from three women, with the following history: The first one had been removed on March 29, at the University Hospital, from a woman who had borne two children by her first husband, but had not conceived since her second marriage, seven years ago. The cyst was of the right ovary, weighed about twenty pounds, and its lower portion had to be enucleated from the broad ligament, which overlapped it. It had burst a few hours before the operation, and the abdomen was filled with a dark, syrupy fluid. The special point of interest was the papillary growths found in large numbers both on the inside and the outside of the cyst wall. The broad ligament and pelvic peritoneum were also studded with them, and the left ovary, otherwise healthy, was so bound down and enveloped with them as to make its removal impossible. The woman's convalescence was uninterrupted, yet Dr. Goodell could not but believe in the malignancy of the cyst, and he was disposed to attribute the diffused patches of papillomata to infection from some previous rupture of the cyst wall, of which, however, there was no history.

The next two cases were operated on at his private hospital on the same day, April 12, and both did well. In each one both ovaries were affected with papillary growths, while in one of them the right cyst had evidently burst some weeks previously, as the abdomen contained a dirty fluid, and apart from the history of sudden abdominal pain, the ovary was represented by a mass of papillomata about the size of one's fist, around the base of which were the remains of the cyst wall. These two cases he classed benign because there seemed to be no infection outside of the ovary. They were probably cases in which the degeneration began at the hilus of the ovary.

The President remarked that these growths were found in almost all small ovarian cysts, especially when they were enveloped in the broad ligament. They are not malignant, but are of very rapid growth and are accompanied by pain and local congestion.

Dr. Parish, in one of his patients, had found an ovarian tumor as large as a man's head. The other ovary was the size of a hen's egg, and its walls had undergone calcareous degeneration. The pedicle was short and calcareous, suppuration followed its removal, and a fistulous opening was left, probably depending on the presence of the calcareous matter.

Dr. Goodell also exhibited a calculus weighing over one and a half ounces, which he had removed a week ago. The lady was seventy-four years old, and was also aphasic and paralyzed from a stroke of apoplexy received five years previously. Ether was therefore given with extreme caution, and fortunately no bad results followed its use. Finding the stone to be a very large one, Dr. Goodell decided not to crush it, but to remove it by lithotomy. This was accordingly done, but a good deal of difficulty was experienced in coaxing the stone through the opening. On account of the cystitis present, the opening into the bladder was left unclosed, except at three points where sutures were introduced merely to stop troublesome hæmorrhage. The relief was immediate and the convalescence uninterrupted.

Dr. W. H. Parish reported an

ALEXANDER'S OPERATION.

In December, 1881, Dr. William Alexander, of the Liverpool Parish Infirmary, performed for the first time what is now known as the Alexander operation. His patient had a retroflexed and prolapsed uterus and a cystocele. He effected a permanent replacement of the uterus, but did not get rid of the cystocele. In 1884 he published his book on "The Treatment of Backward Displacements of the Uterus, and of Prolapsus

Uteri by the New Method of Shortening the Round Ligaments." He there reports twenty-two cases operated on by himself, and other cases operated on by other surgeons. The conditions warranting the operation—conditions which the operation is expected to relieve—are posterior displacements, retroflexion or retroversion of the uterus. The operation is of little service in prolapse unaccompanied by either posterior displacement. It does not remedy a cystocele or a rectocele; neither does it take the place of an operation for remedying a laceration of a perinæum. The operation as recommended by Dr. Alexander consists in making an incision down upon the external abdominal rings, finding and seizing the ends of the round ligaments, drawing them out until taut, the uterus being at the time held in a position of anteversion by means of a sound in its cavity. The ligaments are then stitched in their taut condition to the columns of the rings and the uterus kept from dragging too greatly on the round ligaments during the subsequent two months by means of a Hodge pessary, sometimes aided by a stem pessary. The abdominal incision heals generally by granulation, rarely by first intention. The relief of the uterine displacement in most of the cases reported seemed permanent; at least, the displacement had not recurred up to date of publication. The operation is a bloodless one, or nearly so, the dangers are trifling, and the results in favorable cases were satisfactory in the hands of Dr. Alexander and other English operators.

On December 30, 1884, I did the Alexander operation on a patient in the Philadelphia Hospital. Her general condition was not very favorable for any operation. She was about 40 years of age, of small stature, and in a debilitated condition. She suffered with a chronic bronchitis, the uterus was in a condition of retroflexion and retroversion, and was also so prolapsed as to approach ordinarily the vaginal outlet, and when engaged in severe labor protruded, according to the patient's statement; but the latter statement may have been erroneous, as there was a rectocele of about the size of a hen's egg. There was also an old laceration of the perinæum, one that extended nearly into the rectum. I performed the Alexander operation because of the posterior displacement. I did not expect to remedy, except to a limited extent, the prolapse of the uterus, or at all to get rid of the rectocele. I expected that a subsequent operation would be performed for the restoration of the perinæum.

After cutting down upon the right external ring, I found no difficulty in recognizing the terminal portion of the round ligament or in causing it to run through the canal when drawn upon by the finger; it was about the size of a crow quill and did not suggest strength; it appeared in a condition of atrophy from the stretching to which it had long been subjected. The left ligament was also easily recognized, but it would not run through the canal. It could not be drawn out, and doubtless was adherent to the canal, and also held in the pelvis by abnormal adhesions dependent upon a pelvic cellulitis of old date. The right ligament was drawn out about two inches and cut off. The left one was shortened about half an inch. While the ligaments were being drawn out the uterus was held anteriorly in the median line by means of a uterine sound. The round ligaments were then stitched to the columns of the rings, and the abdominal incision closed with silver wire and covered with iodoform dry dressing. A Hodge pessary was introduced, and the patient placed in bed in dorsal recumbency with thighs flexed. The bronchitis was aggravated by the ether, and severe coughing became very troublesome. The concussion of the coughing would at times force out the pessary. The abdominal adhesions healed in about three days by granulation, after limited erysipelas had appeared. The erysipelas was not severe, and was probably dependent upon hospital influences. The posterior displacements were partially relieved, but I do not think the benefit secured to the patient amounted to a great deal. I have seen but one other Alexander's operation reported in this country. I think the value of the operation should be tested. I am not enthusiastic in its favor, but am willing to give it a trial. Certainly it is not difficult; perhaps the chief difficulty consists in recognizing the round ligaments, but they are more easily found in the living than in the cadaver. There is no danger of hæmorrhage, and apparently no danger of traumatic peritonitis. Probably the only source of danger is from septic infection, and this, under favorable surroundings, could scarcely occur.

The PRESIDENT thinks that the operation will find many cases in which it will be valuable; for instance, cases of retroflexion without adhesions, and in which the pessary will get into the angle of the flexion and lose its usefulness, and in which a stem will not stay. It will be less dangerous than Tait's plan of laparotomy and stitching the uterus to the abdominal wound.

DR. PARRISH remarked that Dr. Alexander recommended that both round ligaments should be drawn out before either of them were stitched.

DOMESTIC CORRESPONDENCE.

LETTER FROM PHILADELPHIA.

(FROM OUR OWN CORRESPONDENT.)

Annual Conversazione at the College of Physicians—Sketch of the College—The Mütter Museum—The Library of the College—Commencement at the University—Election of Emeritus Professor of Physiology—Regulation of Vivisection—Resignation of the Professor of Anatomy in the Jefferson Medical College.

On the evening of April 16, there was held at the College of Physicians of this city what was called an "Annual Conversazione"; in plain English, a meeting at which scientific and social en-

tertainment were blended. For the scientific part Dr. S. Weir Mitchell and Dr. Edwin T. Reichert exhibited specimens of snake poisons and drawings showing their effects; Dr. E. O. Shakespeare and Dr. H. F. Formad showed apparatus for sterilizing organic infusions, various cultures of bacilli, and the bacillus of tuberculosis and of cholera, etc., under the microscope; Dr. J. Gibbons Hunt exhibited other microscopical specimens, and Mr. Holman showed the circulation of the blood with the lantern microscope. Afterward there was a supper. During the whole time the Mütter Museum and the library of the college were open for inspection. Besides the Fellows of the college, there were present a large number of invited guests, including distinguished medical men from other parts of the country and distinguished laymen. The whole affair was brilliant and successful, and calculated to accomplish its purpose, namely, to interest and intertain the Fellows of the college and the guests, and to increase the general knowledge of the character and objects of the college. This institution is not as well known as it ought to be, even in this city, and much less throughout the country. Its history and present resources are deserving of a more extended notice than I can give in a letter; but I think it will interest your readers to learn something about them, as they have a much more than local importance.

The College of Physicians of Philadelphia (which is not a teaching institution, but a "college" in the old and classical sense of the term) was founded in 1787, and incorporated in 1789. The list of names of the founders includes those of John Redman, William Shippen, Adam Kuhn, John Morgan, Benjamin Rush, James Hutchinson, Robert Harris, Nathan Dorsey, and Caspar Wistar. The objects of the college were to study the facts of medicine, to accumulate information from this and other countries, and to become a centre of scientific and ethical culture. In an address delivered in 1884 by Professor Alfred Stillé, at that time president of the college, containing an excellent history of its doings up to that time, the statement is made that though it experienced periods of diminished activity, "yet, if the list of the Fellows is examined at any period, it will be found to contain the names of nearly all the physicians of Philadelphia who possessed unusual merit." And a glance over the whole list shows such names as Hodge, Chapman, Parrish, Hartshorne, Charles D. Meigs, George B. Wood, Gerhard, Pancoast, Condie, Mütter, Samuel Jackson, Carson, Kirkbride, Pepper, Goddard, Washington L. Atlee, McClellan, Gross, and many men still living and as truly distinguished, whom to name would not be fitting at present. Besides this the list of Associate Fellows has always been most honorable. The consequence has been that in this city it has always been regarded as a mark of distinction to be a Fellow of the college, and it is the ambition of every earnest and industrious student to have his own name enrolled after those of such honored predecessors. Notwithstanding this fact, the College of Physicians has never been regarded as a source of extensive literary productions. The mass of medical papers emanates from the more purely literary societies, in which the writers are assured of either a larger audience, as in the County Society, or a selected and specially sympathetic one, as in the various special societies. The College of Physicians has been rather a conservator of the most conservative principles of the profession, the maintainer of the highest standard of qualification for its members, and the goal of a proper medical ambition. Its comparative cautiousness is explained and warranted by the fact that it possesses, not only the proud inheritance of its traditions, but also a very valuable property.

Either of these possessions would justify great care in selecting those to whom the privileges of fellowship should be extended. Of the former no more need be said, of the latter much more than there is space for might be said. The property of the College of Physicians consists of a fire-proof building, a museum, and a library. The building is at the southeast corner of Locust and Thirteenth streets. It is now two stories high and is soon to be increased to three stories to accommodate the growing library and museum. The museum is that known as the "Mütter Museum," from the name of its founder, the late Dr. Thomas D. Mütter, professor of surgery in the Jefferson Medical College, who gave his valuable pathological collection, models, charts, etc., used in his lectures, to the College of Physicians, together with an endowment of $30,000, which was to be devoted to the preservation and increase of the museum. This fund has been so wisely used that the present value of the museum is very much greater than it was originally. It now contains, for example, the very valuable collections of the late Professor Hyrtl, of Vienna, including his marvellous and unique "corrosion preparations" of the vascular system. There is also here his collection of 140 skulls, representing the various races and tribes of the eastern hemisphere, as well as his very interesting collection of the ossicles of the ear from 170 different animals, including those of man. The museum also contains a collection of beautiful preparations and pictures illustrating the diseases of the ear, made by Professor Politzer, and purchased at a cost of $800. Other models and specimens illustrate diseases of the skin, eye, etc. These number 230, and were made by Baretta and Tramond, of Paris, and cost about $2,000. There is also a fine set of sections of the brain, made by Dr. H. D. Schmidt, and mounted in glass cells; while among the curiosities of the museum is the skeleton of a "Kentucky giant," eight feet in height, and a plaster cast of the Siamese twins, while their double liver, and the connecting band between them are preserved in alcohol in a large jar.

Besides the additions which have been made to the Mütter Museum by purchase, it has been enriched by gifts of specimens, and of collections, among which may be mentioned those of Professor Henry H. Smith, and Professor W. S. Forbes, so that it is rich in specimens of vesical calculus, fractures, etc., as well as in models of normal structures. All this is free to anyone who is interested in the study of anatomy and pathology. The museum is open daily, and the Curator—at present Dr. Hinsdale—will offer every civility to those who visit it.

The library of the College of Physicians is, next to the library of the Surgeon General at Washington, the largest and finest medical library in the United States. It now contains over thirty-three thousand volumes, and over twelve thousand pamphlets. It is rich in rare and valuable books, and offers a treasure for research which can only be properly appreciated by those who have tested its resources. In addition to the books here on the shelves, the library can draw upon that at Washington, having such relations to this that any book can be obtained from the Surgeon General's library for the examination by any Fellow, or by any person for whom the college is willing to be responsible. And this brings me to the feature which makes this library of interest to the whole profession. Its use is not restricted to the Fellows of the college. Any person who comes properly recommended can use it. It is absolutely free to all, under suitable regulations. This fact is becoming better understood than it used to be, and the number of those who avail themselves of the opportunities for research which this library offers is continually increasing.

The present size of the library is mainly the result of a very rapid growth during the past twenty-five years. Although the idea of forming a library was proposed as early as in 1788, yet after various vicissitudes the whole collection, in 1836,—nearly fifty years later,—numbered only two hundred and ninety-one volumes. In 1846 the number was about six hundred; in 1855, seventy years after it was started, it comprised only seventeen hundred volumes. Several large donations and bequests now came to swell the number of books, and, in 1863, when the college entered its present building, it owned over four thousand volumes. In 1864 and 1865 Dr. Samuel Lewis gave to the college twenty-five hundred books, a gift which has been followed by continued benefactions, until at present there are in the library over eight thousand books which have been presented by Dr. Lewis, many of them extremely rare and valuable, and all selected with the greatest care and judgment. In addition to the valuable gifts of books which he has made, it would be hard to estimate the worth to the college of the voluntary supervision and fostering care which Dr. Lewis has bestowed and still bestows upon the whole library.

In 1866 the library was first made easy of access by the appointment of a paid librarian, who should be in attendance daily—the work of distributing the books before this having been performed gratuitously and irregularly. The first librarian, under this arrangement, was Dr. Robert Bridges, whose once familiar form and gentle manner are now no longer among the attractions of this, or any other earthly place. How rapid has been the recent increase of the library may be gathered from the fact that last year there were added to it over seven thousand volumes, and that in the first three months of this year there have been added over thirteen hundred new books. These large figures have been made up by special gifts, the former including the library of the late Professor Gross (about 5,000 volumes), which was bequeathed to the Academy of Surgery and permanently deposited with the College of Physicians, and a gift of nearly a thousand volumes by Professor Alfred Stillé; while the latter includes a gift of nearly a thousand volumes by Dr. I. Minis Hays. The average regular addition to the library is considerably over a hundred volumes a month. Putting all together, then, the library at present contains more than thirty-three thousand bound volumes and over twelve thousand pamphlets, the whole catalogued and most conveniently and accessibly arranged in handsome, well lighted and well ventilated rooms, in a fire-proof building. One thing only remains to be said about the resources of the library, which is that there are on its tables at all times over 250 current medical journals, from all lands where there is any medical science or experience: Greek, Russian, Swedish, Danish, German, French, Spanish, Italian, English, and American, all are to be found here. When I repeat that the whole is at the service of anyone who desires to avail himself, or herself, of its benefits, need I apologize for occupying so much of a letter with an account of it and the College of Physicians which has gathered and which maintains it?

We have had nothing of much moment except the commencement of the University of Pennsylvania, at which 107 men took the degree of M.D., and 49 the degree of D.D.S. Professor Agnew made the parting address, and charged the new men to stand by the old code.

The more recent events in the University are the election of Dr. Harrison Allen *emeritus* professor of physiology, and the adoption of rules to govern the practice of vivisection, which are intended to place the practice on as humane a basis as possible. The rules adopted are similar to those issued by Von Gossler, the Prussian Minister of Education and Medicine, and are founded on careful inquiry into the practice at the German Universities. In thus regulating the practice of vivisection in the University, the trustees request the professors to see that the rules are strictly enforced, and will authorize the faculty to refuse the privileges of the laboratories to persons who violate the rules. C. W. D.

SPONTANEOUS RUPTURE OF HYDROCELE.

ED. JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION:

Dear Sir,—Tom K. called upon me with quite a large bi-lateral hydrocele—equally as large as any that I had ever seen. I removed the fluid from one side in the usual manner, afterward using the compound tinct. of iodine, which restored the condition of the sac to its normal calibre. I had intended to operate upon the other during the following week, but Tom failed to come to my office until after the accident, which happened while at work in one of our theatres. He says that he heard the gurgling splash, at the same time noticing an immediate collapse of the scrotal sac, yet no flow of fluid externally visible. He suffered from considerable soreness for several days subsequent to the rupture, and upon his reporting to me I found the condition of that scrotal sac undergoing the constitutional phenomena identically as if it had been punctured, and iodine had been injected into the sac.

No inconvenience to the patient has ever developed since the spontaneous rupture, and he is at his former occupation (an engineer).

Very respectfully,
GEORGE N. MONETTE, M.D.

NEW ORLEANS, May 14th, 1885.

MISCELLANEOUS.

THE MICHIGAN STATE MEDICAL SOCIETY will hold its Twentieth Annual Meeting at Port Huron, commencing Wednesday, at 10 A.M., June 10th, 1885. The members of the Society, and all other physicians in Michigan who can comply with the requirements of the Society, are cordially invited to attend this meeting and identify themselves with its interests.

THE OHIO STATE MEDICAL SOCIETY will hold its Fortieth Annual Meeting at Dayton, June 3rd, 4th, and 5th, 1885. The meeting will be called to order on Wednesday, June 3rd, at 2 P.M. A good programme of work is promised, and a full attendance is expected.

NEW YORK STATE MEDICAL ASSOCIATION, THIRD DISTRICT BRANCH, will hold its First Annual Meeting at Elmira, N. Y., on Wednesday, June 10th, 1885. An interesting list of papers is on the programme, and the meeting will be one of importance.

PROFESSOR P. L. PANUM, of Copenhagen, who presided over the recent International Medical Congress in that city, died suddenly, May 2nd, 1885. An interesting biographical sketch of Professor Panum was given in this journal only a few months since, in which it was announced that he would visit this country in 1887. His death will be much regretted on both sides of the Atlantic. He had attained the age of 64 years, and was highly esteemed by all who knew him.

THE MOST SENSITIVE AND CREDIBLE TESTS FOR ALBUMEN.—At the close of an article on this subject, DR. HENRY B. MILLARD, of New York, draws the following conclusions: That nitric acid shows 1 part of albumen in 100,000. Heat shows 1 part in 100,000, but rather more clearly than nitric acid, and in examinations of urine I often find it to show minute quantities of albumen where nitric acid does not. Tanret's test and my own test will show 1 part in 300,000—the latter test the more clearly; this precipitates fewer of the alkaloids than Tanret's.

Nitric acid and heat show almost exactly the same reaction and percentage with artificial albumen and albuminous urine. Tanret's test and my own show the reaction better in the urine than in the artificial preparation. I think, for practical purposes and ordinary clinical use, we may show with ease, by nitric acid, 1 part in 100,000; heat, 1 part in 100,000; Tanret's test, 1 part in 200,000; the phenic-acetic acid and potash test, 1 part in 200,000; heat showing it more clearly than nitric acid, consequently being more sensitive, and my own test showing it more clearly than Tanret's.

Heat, although somewhat more sensitive than nitric acid, is often quite unreliable from the turbidity produced by it with mucin, and this particularly after acetic acid has been added.

Finally, there are cases in which no single reagent is sufficient, and in which in order to determine the presence of albumen, the employment of several is indispensable.—*Medical Record*, April 4, 1885.

THE PHILADELPHIA POLYCLINIC.—Dr. B. F. Baer, president of the Obstetrical Society of Philadelphia, and late demonstrator of diseases of women in the University of Pennsylvania, has been elected professor of diseases of women in the Polyclinic. Dr. H. Augustus Wilson has been elected professor of fractures and dislocations in the same institution.

RACHITIS AND RHACITIS.—After an examination of the etymology of the proper term for rickets, the New York *Medical Journal* concludes that the word should be spelled *rhachitis*; that the dropping of the "h" is only justified by laziness.

MEASLES IN BROOKLYN.—It is stated that there have been 1,700 cases of measles in Brooklyn since January 1, and that seventy deaths have occurred from it.

A FRENCH EDITION OF LUSK'S MIDWIFERY.—M. Doléris has recently completed a French translation of this valuable work, and a copy of it has been presented to the Académie de Médicine on his behalf by M. Charpentier.

DR. WM. H. PANCOAST has announced his intention of resigning the position of professor of anatomy in the Jefferson Medical College.

THE INCREASE OF INSANITY AND INEBRIETY.—The *Quarterly Journal of Inebriety*, for April, 1885, gives the following statistical facts:

"From the last census it appears that insanity increased one hundred per cent. from 1870 to 1880. This was not the actual increase of insanity, but represented in part the more perfect registration of cases. The whole number of insane in the United States was estimated at one hundred thousand, approximately; the actual number would be more if a perfect record of all the insane could be made. For the treatment of these unfortunates the country has provided eighty different state asylums, and forty private asylums, with a capacity for forty thousand inmates. In reality, fifty-three thousand insane are cared for in these asylums, leaving forty-seven thousand outside, uncared for, and without treatment.

"There are approximately five hundred thousand inebriates in the United States, suicidal maniacs, unknown and unrecognized. There are only twenty-one asylums and homes for their cure and treatment. In these places less than four hundred cases are being cared for. The state punishes by fine and imprisonment not less than sixty thousand of this number every year, with no other result than to precipitate them into more uncurable conditions. The efforts of moralists of necessity fail, because founded on a wrong conception of the nature of inebriety. The mortality of inebriates exceeds that of the insane, and has been estimated at from fifty to sixty thousand a year. The average duration after inebriety has appeared is ten years. The best and most accurate statistics of experts in the study and treatment of inebriates indicate that from thirty to fifty per cent. are curable. Inebriety is without doubt increasing. Some authorities estimate this increase at fifty per cent., others less, but all agree that it is far beyond the ratio of increase of the population."

A NEW METHOD OF ANÆSTHETIZING.—Under this title, M. COLUMBEL (*Lyon Méd.*, No. 41, 1884) recommends the injection of a Pravaz syringeful (1 to 1.5 grammes) of the following: ℞. Atropiæ sulph., 0.02; morph. hydrochlor., 0.20; aq. destillat., 20.00. This to be given twenty to twenty-five minutes before the chloroform. The atropia lowers the irritability of the pneumogastric, and prevents the inhibitory effect of the chloroform on the heart. Eight hundred cases have been successfully anæsthetized by this method in Lyons.—*The Medical Record*, April 4, 1885.

[This so-called new method has been used by some American surgeons for fully three years—possibly five.]

QUACKS IN AUSTRALIA.—The following, taken from the Australian *Medical Gazette*, would seem to indicate that the quack has reached the antipodes. "As we think such superhuman genius should not pass unrecognized, we print the following advertisement from the *Hughenden Ensign*, Northern Queensland. We fear, however, that we shall raise unworthy feelings of envy at the uninterrupted success of this, by his own account, unrivalled practitioner: Dr. A. E. Byrn, physician, surgeon, oculist, accoucheur, and apothecary, late of Victoria and Northern Queensland, is the only surgeon in all the Colonies who treated forty-seven cases of diphtheria without a single death, or performed all kinds of operations without one mishap during a continuous hospital practice of seventeen years. Also surgeon for twelve years to five societies, numbering over 1,600 in families (see their stamped testimonials)."

THE SALE OF COMPOUND MEDICINES IN FRANCE.—The *National Druggist* states that several French courts of law have lately ruled that according to the French law no compound medicine may be sold, wholesale or retail, by any persons other than those holding diplomas. The law says that such compounds "shall not be sold or offered for sale," and makes no distinction between wholesale or retail sale. This decision may concern some firms having agencies in France.

RED QUININE.—The *National Druggist* suggests that, in order to prevent the possibility of mistaking morphine for quinine, the latter drug be tinted red. This plan has its merits. It is obviously better than the plan now in vogue, which leaves the discovery to the man who buys the drug. Painting quinine red is obviously cheaper than coroner's inquests.—*Puck.*

THE NATIONAL PARK AT NIAGARA.—The bill providing for a free park at Niagara Falls has passed both houses of the New York Assembly.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM MAY 16, 1885, TO MAY 22, 1885.

Lieut.-Col. Edward P. Vollum, Surgeon, granted leave of absence for three months, to take effect when his services can be spared by his department commander. (S. O. 110, A. G. O., May 14, 1885.)

Captain A. A. DeLoffre, Assistant Surgeon, relieved from duty at Ft. Sessitor, D. T., and ordered to Ft. Totten, D. T. (S. O. 52, Dept. Dak., May 14, 1885.)

Captain Louis Brechemin, Assistant Surgeon, ordered for temporary duty at Ft. Omaha, Neb. (S. O. 44, Dept. Platte, May 18, 1885.)

First Lieut. Benj. Munday, Assistant Surgeon, relieved from duty at Ft. Klamath, Oregon, and orded to Ft. Walla Walla, W. T. (S. O. 72, Dept. Col., May 12, 1885.)

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE HOSPITAL SERVICE, FOR THE WEEK ENDED MAY 23, 1885.

Mead, F. W., Passed Assistant Surgeon, detailed as member of board for physical examination of candidates for appointment as cadets in the Revenue Marine Service. May 18, 1885.